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All communications should be addressed to the Editor.

No. 98. FEBRUARY, 1919. VOL. XXIII.

Notices of the Royal Aëronautical Society.

Elections.

The following have been elected in the various grades of membership:—

Associate Fellows.—O. G. C. Drury, M. H. Hutchinson, E. G. Kright, J. E. G. Lahey, J. L. Prichard, A. H. Tiltman, F. W. Halliwell.

Students.—A. G. Bocking, H. E. Boyle, C. P. B. Brazier, A. Burdett, M. P. Burke, I. H. Clark, W. F. Field, A. H. Fox, S. Goodfellow, N. L. Hines, G. H. Howgate, R. H. King, A. S. R. Lewin, L. Onions, R. Peacock, L. Pinder, R. A. Pomphrett.

Associate Members.—J. K. Baughan, A. A. Brooks, W. H. Crossland, I. Davson, C. H. Masters, V. H. Mears, A. Pickern, H. I. Sidgwick, F. H. S. Rasch, N. R. Temperley, J. A. Todd, F. E. Whiting, H. F. Woodley, F. Wright.

Members.—A. A. Collis, J. P. B. Jeejeebhoy, R. Loraine, G. Le M. Mander, H. W. Mirehouse, K. Robins, C. H. Chichester Smith, A. E. Turner.

Foreign Members.—K. Austin, T. N. Johnson.

The Annual General Meeting

Will be held in the Offices of the Society on April 2nd. The business consists of the presentation of the Annual Report and Balance Sheet and the election of ten members to vacancies on the Council. Sixteen nominations in due order have been received.

FULL SCALE AEROPLANE EXPERIMENTS.

BY W. S. FARREN.

SUMMARY.

INTRODUCTION.

The objects of full-scale experiments.

PHYSICAL STATE OF THE ATMOSPHERE.

The weather difficulty.

SPEED.

Comparison of direct and indirect methods of measurement; stalling speed.

RATE OF CLIMB.

Recording *v.* non-recording instruments; partial climbs; allowance for up and down currents.

AEROPLANE RESISTANCE.

Engine power; propeller efficiency; thrust and torque meters; glides; direct measurement.

AIR PRESSURE DISTRIBUTION.

On wings, etc.

AIR SPEED EXPLORATION.

In the air near the aeroplane.

CENTRE OF PRESSURE OF WINGS.

Direct measurement.

STABILITY.

Difficulty of finding a criterion; methods of recording periodic motion of aeroplane; distinction between stability and trim; lateral stability experiments.

CONTROLLABILITY.

Radius of turn; lateral control; personal preferences; accelerations in flight.

ORGANISATION OF FULL-SCALE AEROPLANE EXPERIMENTS.

Aeroplanes; construction and repair facilities; experiment and production; personnel.

The subject of full-scale aeroplane experiments is one which I am sure is of interest to all concerned in aeronautical engineering. The interest of the present paper would be increased if it were possible to include in it detailed descriptions of actual experiments, numerical results, and a discussion of their bearing on design. It will be realised that this is impossible. With so much to choose from each of a number of experiments would deserve fully the time which I have to divide among them all, and a paper with this title can only be a survey of the whole field, dealing with certain general questions of importance.

Up to the present time most of the full-scale aeroplane experiments have been carried out at the Royal Aircraft Establishment. Their cost has fallen, eventually, on the country at large. It is only right therefore to record here, what is of course a commonplace, that wherever the results of these experiments have been considered by the Technical Department of the Department of Aircraft Production to be of value, they have been made accessible to all aeroplane designers—of

course under proper conditions as regards secrecy. Anyone, therefore, who feels that when I get down to details, I stop just where he most wishes to go on, will realise that the people who really need the results have got them.

The two main objects of full-scale research in any branch of engineering are:—The establishment of satisfactory design formulæ; and the investigation of new phenomena and their explanation in terms of existing or new formulæ. "Full-scale" is, of course, a relative expression. I have myself been intimately concerned in the design of aeroplanes varying in total weight from 350 to 1,500 lbs., and in wing area from 100 to 2,000 square feet. I am not aware of any "model" aeroplane of more than about 6 square feet in wing area, but the time is not far distant when the gap between 6 and 100 square feet will be at any rate partially filled up, as the increasing size of wind channels permits the use of larger models. In so far as any definition is necessary, I think it may be said that a full-scale aeroplane is one which is capable of being put to the ordinary uses for which aeroplanes exist—the chief one being to carry a man.

In no engineering structure is there so little to spare as in an aeroplane. It is therefore absolutely necessary to use every means we have of bringing every detail of its design down to firmly established scientific methods and to leave nothing whatever to guesswork. Full-scale aeroplanes being, as I shall endeavour to show, somewhat unsuited by nature for accurate experiments and being moreover very expensive things to construct and maintain, naturally the information required was sought from models. At the National Physical Laboratory and elsewhere this work has been carried to a very high state of development and the technique of model experiments leaves nothing to be desired. But in applying the results of them we are faced with a question which imperatively demands some answer. Can these results be applied direct and without modification? In my opinion, the fullest and most logical statement of the case can be found in the Report of the American National Advisory Committee for Aeronautics, 1917, by Dr. Durand. He shows that no *a priori* answer can be given. Experiments alone can decide. We are faced then with the necessity of undertaking full-scale experiments in order to enable us to use model ones. In fact, the design formulæ which we deduce from model experiments cannot be considered to be established until they have been checked by full-scale work.

I am tempted to take up a little more time in elaborating this subject, but I see that in the list of papers for this season is one entitled, "From Model to Full-Scale in Aeronautics," by Mr. H. Levy, of the National Physical Laboratory, and I do not wish to trespass on the ground he will probably cover. As, however, he will probably look on the subject from a point of view which is rather different from my own, I wish to say just this: Anyone entering the field of full-scale aeroplane experiments is apt to consider himself, in my opinion erroneously, in a position not unlike that of a schoolboy who has a text-book of, say, arithmetic, in which are sets of examples which he has to work out. The answers to those examples are put in an appendix at the end of the book. For the good of that boy's soul, the master makes him seal up those very useful pages. It would be foolish for us to take the analogous course of treating the results of model experiments as a sealed book—for the simple reason that they are not really answers to the questions we have been set to work out. Moreover, the model results can be of very great value in suggesting lines of thought and action. But there is no obligation of any kind upon us to make our answers *agree* with the model experiments. Our whole work is to devise and carry out experiments whose results must be capable—and worthy—of being *compared* with the model results. We are faced with difficulties immeasurably greater than those of model experiments and we may be forced to use statistical methods of analysis where the "model" man would scorn anything but clear cut, direct methods. I think, however, that sufficiently good results have been obtained and that a direct comparison with model results is legitimate. We should not prejudge the issue

or feel that there is necessarily something wrong with the full-scale work when the agreement with the model results is not as good as we hoped it might be.

Over and above this work there is an immense field which, so far, has only been broken into, and its problems are the most fascinating of all. I refer to the investigation of phenomena which cannot by their very nature be tackled from the model side except in certain special cases. Stability, controllability, manœuvres of all kinds; all the questions of that very elusive "feel" of an aeroplane.

I propose to deal briefly with some of the important points in these two divisions of the work and then refer to some of the questions involved in the organisation of the work.

THE ATMOSPHERE.

The whole essence of experiment is measurement. Certainly one of the most important measurements we have to make—and almost the most difficult—is that of the physical condition of the atmosphere. We have, of course, no control whatever over it. We have to take it as we find it. We know little enough about the atmosphere in a general way—regarding it from meteorologists' point of view. As a medium in which experiments are to be conducted it can only be described as very unsatisfactory, especially when it is remembered that ultimately the assumption is always made that it can be considered as equivalent to a uniform still fluid.

We can and do measure the temperature and pressure of what is fairly certainly the free air near the aeroplane. We take precautions to use instruments which are sensitive to fairly rapid rates of change of pressure and temperature. We can also measure the rate of change of pressure directly with good accuracy. But the fundamental uncertainty as to the state of the air generally is hardly affected.

For many experiments it is luckily sufficient if we can find a calm region in the air where the instruments carried on the aeroplane indicate *steady* temperature and pressure. Such a region can generally be found just above a layer of clouds and much use is made of that fact for stability and other similar work, though in these circumstances any observation from the ground is impossible.

For measurements of performance, however, it is important to know that the atmosphere is in a steady state, without up or down currents or rapid alteration in rate of change of temperature with height, over a considerable area. This means clear settled weather. In such weather it is probable that the state of the air generally is not very different from that indicated by the aeroplane instruments. If we take care to exclude results which differ from the mean by more than a certain amount (which can only be decided by experience) it is probable that the loss of accuracy owing to uncertainty as to the state of the atmosphere, in purely test measurements of performance, is not serious. But for the measurements to which I refer a different kind of accuracy is aimed at and I think it is certainly true that our inability to secure a steady state in the atmosphere is responsible for most of the difference between our aim and our attainment.

All this points to the necessity for selecting the time for experiments very carefully. It means that if the weather is suitable for test work on say 70 per cent. of the days of the year, then for research it is probably suitable on about 35 per cent. of the days. Actually it has been necessary to carry out research experiments on say 60 per cent. of the days, resulting in a loss of accuracy. This is a state of affairs which is admittedly due to the war and with the war it should finish.

There are no special difficulties in connection with the instruments which are used to measure temperature and pressure. It is important to eliminate lag,

but this is not special to this problem and I will not go further into the subject. I may say, however, that our resources in the matter of instruments are very good indeed.

SPEED.

The speed of the aeroplane relative to the air is one of the fundamental quantities determining the forces exerted by the air on the various surfaces and much labour is spent on making the measurement of speed as accurate as is possible. Convenience in carrying out the measurement is essential, for, on account of its importance, frequent check calibrations are required. Any method which depends on timing over a measured length on the ground (or any method equivalent to this, *e.g.*, angular measurement from one or more points) involves the velocity of the air relative to the earth. Since it is evidently not practicable to measure the wind velocity at more than a very limited number of points, especially simultaneously, the assumption has to be made that the wind velocity over the whole course can be determined with sufficient accuracy from the one or two measurements which are all that can conveniently be made.

Any other method must involve the use of some device whose operation depends on the relative motion of the aeroplane and the air, and hence the assumption that there is no interference between the aeroplane and the air at the point where the instrument is placed. Experiment shows that such a point is difficult to find and is inconveniently far away from the aeroplane.

It is convenient to divide the methods involving observation from the ground into (A) those in which the aeroplane must be flown within narrow limits over a definite line on the ground, and (B) those in which it is necessary only to pass across a certain line, between two definite points on it, the direction of flight being more or less at right angles to the line. Class A may conveniently be described as "speed course" methods, Class B as "camera obscura" methods, though it is of course quite possible, though inconvenient, to use a camera obscura for Class A. It is evident that other things being equal, Class B is easier for the pilot than Class A.

GROUND SPEED COURSE.

The first method used was what is generally termed a *ground speed course*. The aeroplane is flown within a few feet of the ground—so near that sometimes the wheels have actually touched. The course at Farnborough is 1,000 yards long and practically level. Two observers with simple fixed sights take the time of passing in each direction. From the times, both the aeroplane's speed and the wind speed can be determined. The effect of a light cross wind is eliminated by allowing the aeroplane to drift with it, *i.e.*, the aeroplane is kept parallel with the length of the course.

A simple system of timing is used which very much reduces the likelihood of error due to the personal equation of the observers and at the same time makes the apparatus very simple. Each observer has a stop watch and there is a bell at each station which is operated by a key at the other. When observer A (Fig. 1) sees the leading edge of the wings of the aeroplane on his sight, he starts his watch and presses his key. When B hears his bell he starts his watch. When the aeroplane passes B, B stops his watch and presses his key, and when A hears his bell he stops his watch. Assume that the personal equations of both observers are equal, and that t_1 is the time between the aeroplane's passing a sight, and the starting (or stopping) of the watch at that end, and t_2 the time between the same passage and the starting (or stopping) of the watch at the other end. t_2 is evidently greater than t_1. Then A records a time which is greater than the true time by $(t_2 - t_1)$ and B a time which is less than the true time by $(t_2 - t_1)$.

Hence the mean of the recorded times will be very nearly the true time. A very good check is furnished by noting, for a number of runs, the difference between the times recorded at the entering and leaving stations—this should remain constant, being equal to $2(t_2 - t_1)$, and this difference does remain very nearly constant in practice.

The advantages and disadvantages of the ground speed course may be summarised as follows:—

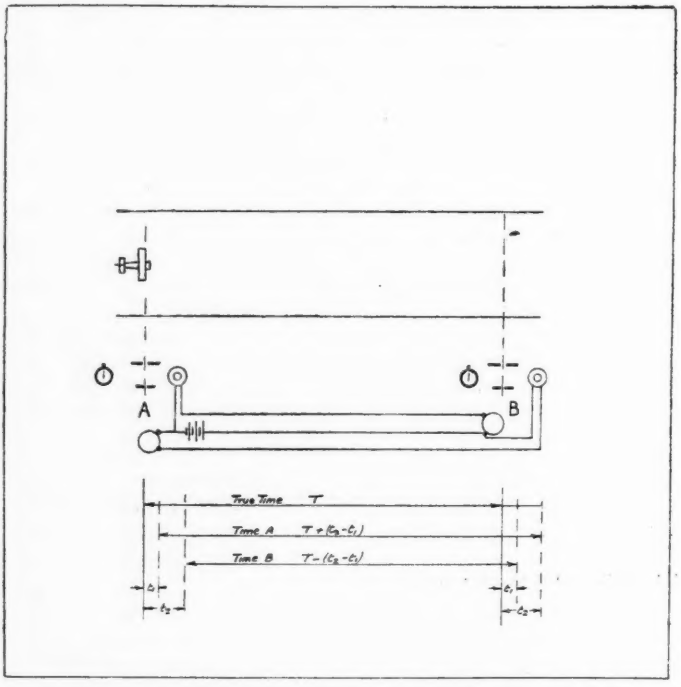


FIG. 1.—DIAGRAMMATIC REPRESENTATION OF GROUND SPEED COURSE.

Advantages.

1. It is easy for the pilot to keep his direction correct and to fly level.
2. The course can be used on cloudy days.
3. The course can be used on a greater percentage of days than a course at two or three thousand feet, because the wind near the ground is almost always less than higher up.
4. By means of automatic recorders the wind speed and direction can be measured very near the course at several points and the observations used to check the value of the along-the-course component derived from the timing of the aeroplane.
5. The instruments are simp'e and the accuracy of observation is good.
6. The organisation and equipment required are simple.

Disadvantages.

1. It is very difficult to maintain the speed steady owing to the necessity for keeping an eye on the ground.
2. It is difficult to find a suitable place for a course of a good length so that the time on a ground course is generally short, and the accuracy poor, for fast machines.
3. It is dangerous in case of engine failure when coming off the course, unless there is space, and there generally is none.

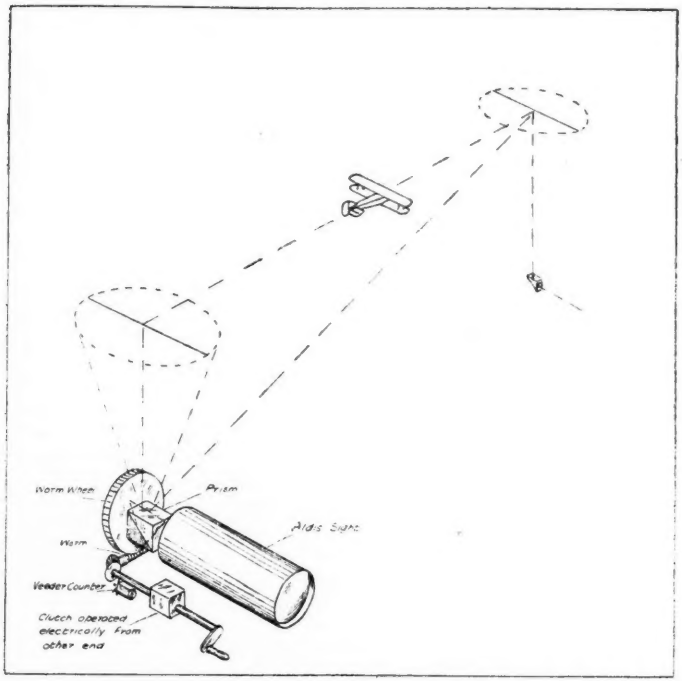


FIG. 2.—DIAGRAMMATIC REPRESENTATION OF HIGH SPEED COURSE.

HIGH SPEED COURSE.

The natural extension of the ground speed course, when it is required to measure speeds at any height above the ground, is *a high speed course*, the ends of which are determined by cross wires in suitably arranged wide angle telescopes. The arrangement of the existing one at Farnborough is shown in Fig. 2. The length is about 4,000 yards. The telescopes give no magnification (*i.e.*, are practically Aldis sights) and have reflecting prisms in order to bring the eye-piece in a position which is convenient for observation. The angular range is about 30° so that at 3,000 feet the aeroplane can be about 800 feet off the course laterally in either direction and still be in the field. An error of this amount in opposite directions at each end would correspond to an increase in the length of course of 0.9 per cent. There is also a probability of an error always in one direction owing to the fact that any actual course must be longer than the assumed straight

one. There is fortunately a very good mark at Farnborough—a large pond—almost in the centre of the course, which helps the pilot considerably. Moreover, there is an attachment by which the reflecting prisms on the sights can be rotated about the axis of the sight, so that the aeroplane can be kept in sight over the whole course, and the deviation from the straight course estimated. It is surprising how small this deviation is on good days (small cross wind) and with an experienced pilot. The main purpose of this rotating mechanism is to measure the aeroplane's height at each end. The mechanism at one end is thrown out of gear electrically by the signal at the other end which records the aeroplane's passage, and a Veeder counter records the angular height directly, so that no error can arise from the short time which would be available for reading a vernier on a scale of degrees. All the times are recorded at one end by means of electrically operated styles on a travelling band of metallic paper.

The most unsatisfactory feature of the high speed course is the measurement of the wind velocity. The first stage in this is the firing of a smoke puff from the aeroplane at one or more points on the course. The second is the observation of the motion of the puffs as they are carried along with the wind.

The smoke puffs are fired from a Very's pistol and many kinds of cartridge have been tried. The ordinary Very's light is unsuitable because of the rapid fall of the light. Special smoke bombs of various types have given moderately good results. Parachute lights proved very successful. The nature of the background (*i.e.*, whether clouds or clear sky) makes a very great difference, some puffs being very good on a clear day and quite invisible against clouds. A *dark* puff appears to be difficult to get. The pistol is always pointed upwards at about 45° to allow for the fall of the charge before it bursts. With practice no difficulty is found in getting the puff within a hundred feet of the level of the machine.

The puffs are observed and their paths traced in a camera obscura. Direct optical methods, using a squared graticule in the eye-piece, will probably prove to be more satisfactory. With two cameras the height of the puff can be measured. With one it is necessary to depend on the observer firing the pistol so that the puff is at the same height as the aeroplane.

At Farnborough there is a camera at the home end of the course. One puff is fired after every double run and one at the beginning, *i.e.*, four puffs in the usual three double runs. How far the mean wind velocity over the whole course differs from the mean of the observations at one end can only be determined by experiment. The mean along-the-course component is of course obtained from the difference between the apparent speeds of the machine out and home, and this acts as a check. An elaborate series of experiments would be needed to obtain more certain evidence. Pressure of work has prevented these being carried out up to the present, but it will certainly be necessary to undertake them in the immediate future.

An extension of the high speed course with two ends has been proposed, in the shape of an additional station, arranged so that the three stations form approximately an equilateral triangle. Flights could then be made in any or all of three directions and components of mean wind velocity in these directions obtained directly. The instrumental difficulties are considerable, but not insuperable, although the working of the course would be complicated.

In connection with the question of the direction of the wind for the best accuracy an interesting point arises. If the wind direction can be determined within two or three degrees, and the speed only within say one mile per hour, then it is best for the wind to be across the course. If the wind speed can be determined with better accuracy than the direction, the best direction is along the course. The latter condition is best for the flying of the aeroplane, but it is probable that actually the former condition is nearer to the truth, a light wind more or less across the course giving the best results.

The advantages and disadvantages of a *high speed course* may be summarised as follows:—

Advantages.

1. It is easy to keep the speed constant.
2. A long course is easily arranged so that the time of runs may be long.
3. It is safer than a ground course.
4. The operation is easy and the results can be reduced rapidly.

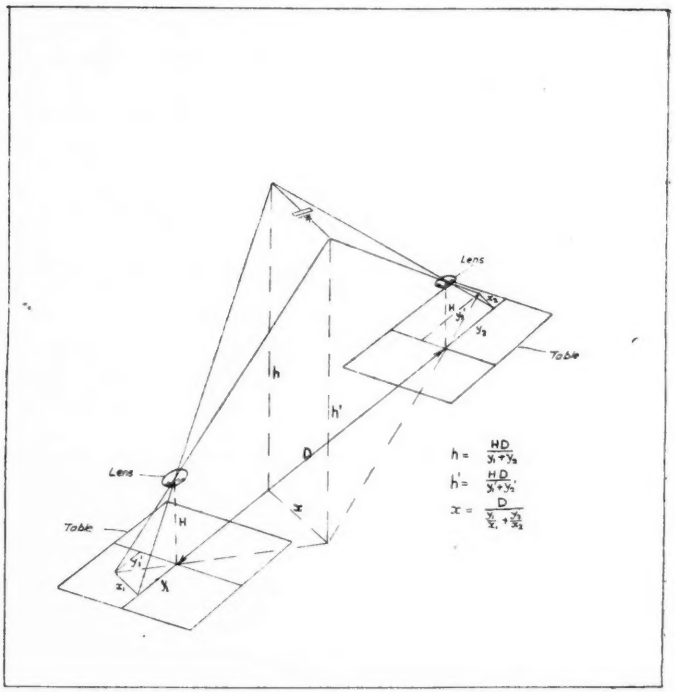


FIG. 3.—DIAGRAMMATIC REPRESENTATION OF TWO-CAMERA SPEED COURSE.

Disadvantages.

1. It is difficult to keep direction.
2. Clouds and wind increase seriously the number of bad days.
3. The accuracy of measurement of the wind velocity is not as good as is desirable.
4. The instruments and equipment are elaborate and there is a considerable loss of time owing to having to transport observers to the distant stations.

CAMERA OBSCURA METHODS.

Turning now to what I have termed "camera obscura" methods, a typical one is that originally used at the Testing Squadron at Upavon (Fig. 3). Two cameras were used (actually their axes were inclined towards one another for convenience) and the path of the aeroplane was traced in with cameras. For good

accuracy the projection of the line of flight should pass between the cameras and the direction of flight should be at an angle of not less than about 45° with the line joining them. From the traces of the aeroplane's path on the tables of the camera, by a fairly simple calculation, both the height and the speed at any point can be obtained. The diagram shows the principle on which the calculations are made. It will be seen that no angles, but only lengths have to be measured.

The wind velocity is measured in the same way as with a high speed course, but the height of the smoke puffs can be determined directly.

Instead of a camera obscura, mirrors can be used, the path of the aeroplane being viewed through pin holes, and its trace on the faces of the mirrors being followed with a pen.

There are many other possible variations of this type of method, but they are all very similar in principle.

The advantages and disadvantages of "camera obscura" methods may be summarised as follows:—

Advantages.

1. Very easy for the pilot to keep a straight course and constant speed and height.
2. Within wide limits the direction of flight may be varied to suit the wind direction.
3. The height can be checked all along the course.
4. Direct measurement of the height of the smoke puffs can be made.
5. The original record of every course can be kept permanently.

Disadvantages.

1. The accuracy with which the path of the aeroplane can be traced is not very good.
2. The measurement of the traces and the reduction of the results take appreciably longer than the corresponding operation with a speed course method.
3. The definition of a camera obscura is materially less than that of a good optical sight and there is a fair percentage of lost days due to this cause only.

Combination of the "speed course" and "camera obscura" methods are quite possible and it is probably among these that the best methods will be found. The methods described cover most of those in actual use and give a good idea of some of the typical troubles of aeroplane experiments. It will be noted that the question of the wind velocity overshadows the whole problem.

SPEED INDICATOR METHODS.

Measurement of speed by an instrument on the aeroplane involves making sure that there is no interference between the aeroplane and the air near the instrument. Reference will be made later to experiments designed to investigate the amount of interference at various points near the aeroplane. To make the method really useful as an absolute measure of speed, it is essential to make such a comprehensive set of experiments, that it will be possible to say in the case of a new type of aeroplane, differing materially from existing types, that in a certain position the interference will be inappreciable. Our present state of knowledge does not enable us to do this. At least, no such position could be specified that would not be ruled out by the very cumbersome supporting gear which would be necessary. It is quite conceivable that a kind of log may be developed which could be trailed behind (and of course considerably below) an aeroplane. The

ordinary pitot head, fixed, say, to a wing strut, is only of use as an accurate speed measuring device when it has been calibrated by some absolute method such as those just described. The speed registered by the ordinary aeroplane instrument, when corrected for density of the air and for instrumental errors, is generally below the true speed by an amount which does not vary much but increases as a percentage as the speed decreases, being generally about 2 to 3 m.p.h. By using a head attached to a wind vane, which can swivel in a vertical plane, the error owing to the yawing of the pressure head due to the varying angle of incidence of the machine can be eliminated, and it appears that the true speed course factor, as it is termed, is almost a constant percentage, being equivalent to about ½ m.p.h. at 100 m.p.h.*

The best method of securing and keeping a good standard of accuracy in speed measurement is probably to calibrate one machine very carefully on a speed course of some type, selecting ideal days, and to use it as a standard for calibrating others. This is colloquially termed a "formation" method, from the fact that the two machines—or more if required—fly very close together for a considerable distance. Undoubtedly very high accuracy can be obtained in this way, as the whole error in distance is only a few feet in several miles. The standard machine flies at a steady indicated speed—the pilot's only duty being to keep the speed steady—and the other machine or machines keep level with it. By proper co-operation it should be possible for anyone doing experiments in the future to calibrate the instrument on his machine by this method against a standard machine at Farnborough.

In connection with the instruments used in measuring speed on the aeroplane the question arises whether the liquid (U tube) indicator is preferable to the dial (diaphragm) instrument. Undoubtedly the latter is very much more convenient, and easier to read rapidly and accurately, but as an instrument it has disadvantages. Frequent calibration accompanied by periodic subjection to a kind of exercise is necessary. The indicator is connected to a piece of rubber tube bent into a U, supported under an ordinary water tap. The U tube is filled with water up to the open end. The air in the other leg is thus compressed so that the needle of the indicator moves round by an amount which can be varied by raising or lowering the open end of the tube. The tap is then turned on and allowed to run so that a broken stream of water is obtained. This impinges on the open end of the U tube with the result that there is a continual and irregular change of pressure on the instrument, which effectively shakes up the diaphragm, and keeps it from becoming set in any one position. This may seem an elementary method, but it has proved effective, and after being subjected to this treatment indicators give more regular results than before.

With careful calibration and constant changing about, particular care being taken not to leave instruments standing unused for long periods, very good results have been obtained with dial indicators, but for very accurate work differential liquid instruments are preferable. These work on the equivalent of a liquid with density equal to the difference of the two liquids employed, and as this can be small the scale can be very open.* These instruments have many disadvantages, being cumbersome and difficult to keep in order. But they are not subject to the same diseases as dial instruments and are preferable for some purposes, though the undoubted convenience of the dial instrument makes it, in my opinion, the best for general work.

STALLING SPEED.

When a speed below about 50 m.p.h. has to be measured accurately a differential liquid velometer is probably superior to a diaphragm instrument. There is no

* These statements are not quite accurate. See Mr. Stevens' remarks (1) and (2) in the discussion to my reply.—W. S. F.

great difficulty in making a diaphragm instrument with an open scale, even for speeds down to 10 m.p.h., but the lag is always very big. For measurement of stalling speed—defined as the lowest speed at which the aeroplane can fly steadily, the total lift being equal to the total weight—a liquid velometer has the additional advantage that, since it is gravity controlled, it should be impossible to make it read below the true stalling speed even if the aeroplane has a vertical acceleration, *i.e.*, if its lift is not equal to its weight. Experiments have been carried out with a differential velometer connected to a swivelling pitot head, but certain unexpected difficulties have been encountered, and the results are not yet available.

Hitherto a dial instrument has been used. With this it is necessary to stall the machine very slowly; in fact, to get really good accuracy, it should be flown steadily at the stalling point, and it is doubtful if it is really possible to do this. The probable error in the results obtained by this method is about 2 per cent. in speed, *i.e.*, 4 per cent. in lift coefficient, and it is not certain that the lift coefficient so determined is really the maximum. Allowing for this amount of error, even the lift coefficients measured up to date are, on the whole, higher than had been expected.

The accurate determination of the stalling speed is of importance, because it is usually taken as one of the starting points of the design of an aeroplane.

I have dealt with the question of speed measurement at some length because I wish to emphasise its importance and to show how much work has been put into making it accurate. I will now turn to the question of measurement of *rate of climb*.

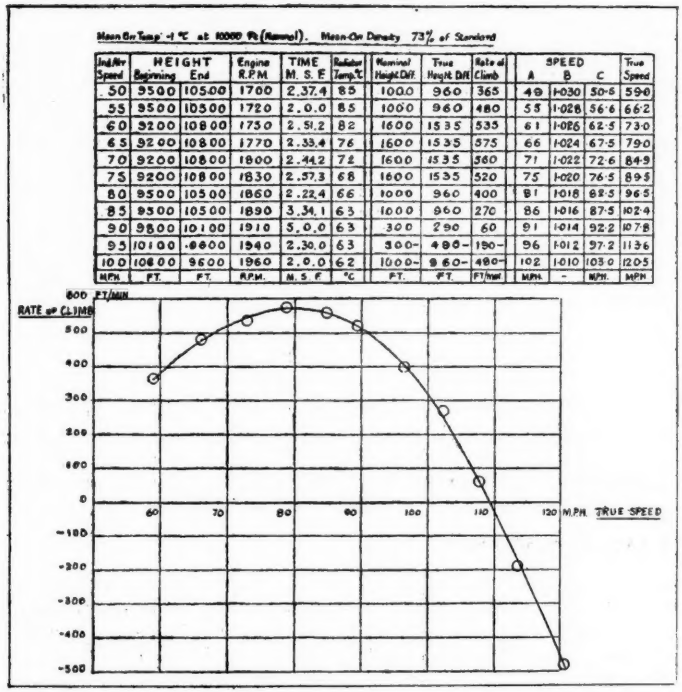
RATE OF CLIMB.

We do not measure rate of climb (or loss of height) directly, but rate of change of pressure and temperature. These quantities have already been referred to in connection with the characteristics of the atmosphere generally. In so far as rate of change of pressure in particular is concerned, there is a choice of three alternatives—an automatic recorder or barograph; an aneroid barometer and stop watch; and a climbmeter, as it is called. The first two do not require any detailed description. The choice between them is almost entirely a question of the supply and maintenance of instruments. I am of the opinion that the aneroid and stop watch method is the most satisfactory in every-day work, but the recording instrument has advantages in connection with single-seater machines. On the whole I think that when the automatic instrument has been developed further on the lines of some instruments I shall mention later it will be superior. But—and this applies to automatic instruments generally—where it is possible to obtain what is wanted with a simple instrument and a good observer, from experience I prefer that combination to the recording instrument. My chief reason for this preference is that the number of lost results is less and the observer in a much better position to keep in touch with what is going on, and to note anything abnormal and to take advantage of it. It is surprising how much information has been obtained in this way which would not have been obtained with recording instruments. In fact often we should not only not have got some fresh information but we should actually have rejected the result as being an instrumental error, or at any rate too doubtful to deserve much weight.

The actual aneroid instrument is very variable. Lag is the chief enemy of all instruments which depend on the deflection of thin diaphragms, and aneroids are no exception. The R.A.E. possesses one or two aneroids which are probably among the finest in existence—that they are so is due to the head of the firm which produced them. I believe he made with his own hands a large number of diaphragms, and only one or two of them came up to the desired standard.

The climbmeter, which was described by Colonel Tizard in his lecture the year

before last, is an old balloon instrument under a new name. It consists of a thermos flask containing air connected to the external air by a very small tube. The changing pressure of the atmosphere causes the amount of air in the flask to change, the internal pressure trying to keep pace with the external. This can only take place slowly on account of the size of the connecting tube. A U tube shows a difference of pressure between the flask and the outside air, which depends on the rate of change of pressure outside. The instrument has been made very compact and the accuracy is good, but it has several objections which are not easy to overcome, the chief ones being the dependence of the scale on the air density and the difficulty of securing a convenient scale for low rate of rise or fall without making the lag big.



Ind. Air Speed	HEIGHT Beginning	HEIGHT End	Engine R.P.M.	TIME M. S. E.	Radiator Temp. °C.	Nominal Height Diff.	True Height Diff.	Rate of Climb	SPEED A	SPEED B	SPEED C	True Speed
50	9500	10500	1700	2.37.4	85	1000	960	365	49	1·030	50·5	59·0
55	9500	10500	1720	2.0.0	85	1000	960	480	53	1·028	56·6	66·2
60	9200	10800	1750	2.51.2	82	1600	1535	535	61	1·026	62·5	73·0
65	9200	10800	1770	2.33.4	76	1600	1535	575	66	1·024	67·5	79·0
70	9200	10800	1800	2.44.2	72	1600	1535	560	71	1·022	72·6	84·9
75	9200	10800	1830	2.57.3	68	1600	1535	520	75	1·020	76·5	89·5
80	9500	10500	1860	2.22.4	66	1000	960	400	81	1·018	82·5	96·5
85	9300	10500	1890	3.34.1	63	1000	960	270	86	1·016	87·5	102·4
90	9500	10100	1910	5.0.0	63	300	290	60	91	1·014	92·2	107·8
95	10100	9600	1940	2.30.0	63	500–	480–	190–	96	1·012	97·2	113·6
100	10600	9600	1960	2.0.0	62	1000–	960–	480–	102	1·010	103·0	120·5
MPH.	FT.	FT.	R.P.M.	M. S. E.	°C	FT.	FT.	FT/min	MPH.	-	MPH.	MPH.

FIG. 4.—TYPICAL RESULTS OF PARTIAL CLIMBS.

NOTE.—*Speed A—Indicated speed corrected for instrumental error.*
B—Speed course factor.
C—Indicated speed corrected for instrumental error and S.C.F.

The mere measurement of rates of change of pressure and temperature does not, of course, suffice to determine the work done against gravity, because of up and down currents. It is almost impossible to suggest any reasonable method for measuring these directly. There are several ways of allowing for them, all more or less objectionable. The best way, probably, is to measure the longitudinal inclination of the aeroplane, and, from previous determination of the angle of inclination for level flight at the same speed, to determine the rate of climb directly from the difference. It is possible, with care and suitable instruments, to measure

to 1/10 degree, which corresponds to about 10 ft./min. at 70 m.p.h. I do not think this method has received the attention it deserves. The precaution usually adopted is, of course, to repeat experiments several times until good agreement is obtained.

In research work we are not much concerned with the time taken to reach any particular height. In an ordinary climb test all the important quantities (true speed, indicated speed, engine revolutions, propeller efficiency) are varied in order to get the maximum rate of climb at each height. This is a state of affairs which is undesirable for research work, where it may be taken as a very good rule that the more things we can keep constant, the fewer will be our difficulties in extracting information about the independent variables. The usual method, which proves to be easy to work in practice, is what are known as *partial climbs*. It simply consists in measuring the rate of climb at, say 10,000 feet, at speeds varying in steps of 5 m.p.h. from the lowest convenient speed to well above the level speed, the latter observations being of course of the rate of *loss* of height. All these tests are done with the engine at full throttle. The actual measurement of rate of climb may be made in a number of ways. The time from 9,500 to 10,500 feet may be measured, or the height gained or lost in any interval of time, the length of which will vary with the actual rate of climb, in order not to put too much work on the engine. A combination of the two methods is generally best, the first being used for moderate speeds when the rate of climb is large, and the second when the rate of climb begins to approach zero. Fig. 4 shows a typical set of results, and the curve shows the deduced rates of climb. With experienced pilots and observers very good results can be obtained. In order to keep the conditions as regards up and down currents as nearly as possible the same, the machine should be flown backwards and forwards over the same area. Repetition of the complete series on other days will serve to show the probable error due to this cause.

I think that a climb meter in which the outlet tube could be closed by a tap, thus converting it into a statoscope (practically a very sensitive barometer) would be useful for partial climbs, especially for single seater machines. The time measured would be that taken by the indicating liquid to move from one definite mark to another. The scale could be very open, and the accuracy would be good as the lag would be negligible. It would be an easy instrument to observe.

AEROPLANE RESISTANCE.

To make it possible to reduce the results completely, information is required as to the *engine power* and *propeller efficiency*. Considerable difficulty has been experienced with the latter quantity in particular. Dealing with it first, the only direct measurement possible is on the Whirling Arm at Farnborough. I must admit that this has not proved a very satisfactory piece of apparatus. I do not intend this as a reflection on the designers—there was no previous experience of whirling arm design from which they could profit. It was a purely experimental piece of apparatus, but owing to its size and cost a policy of patching up had to be followed where one would have preferred to scrap a good deal of it and start again. Nevertheless many measurements of torque, thrust and efficiency of propellers have been made. The highest permissible rotational speed of the arm is equivalent to about 80 m.p.h. at the propeller, so that the full engine power can be put into the propeller under conditions which are very nearly the same as those in flight.

Engine power is taken from test bed measurements. Allowance is made for the reduction in power at high altitudes. If the engine power is known at any one height (*e.g.*, near the ground) it is possible by proper use of observation of propeller revolutions at various aeroplane speeds on full throttle to determine the variation of power with height. Many kinds of engines have been tested and it has been proved that the engine brake horse power varies very nearly directly as the atmospheric pressure and is independent of the atmospheric temperature. This

applies with good accuracy to almost all types of engine and up to all normal altitudes.

There are many important matters connected with engine power which have to be borne in mind—carburation being the chief one—but I do not propose to enter into more details here.

Given engine power and propeller efficiency and the results of partial climbs, the total resistance of the aeroplane can be obtained by simple analysis. Much work of this kind has been done and I think I can say that much valuable knowledge has been obtained. *Direct measurement of the propeller thrust* is of course a better and more accurate method. Our success in this direction has been only fair, the instrumental difficulties having proved very great. Several thrustmeters are now being developed and some are very promising. Torque meters and combined thrust and torque meters have also been considered and it is certain that reliable instruments will soon be developed. These will replace the whirling arm for propeller measurements. The chief difficulty in all these instruments is the transmission of large forces by small parts without incurring errors due to friction. As with the whirling arm, the apparatus is costly—by this I mean rather that it takes many man-hours to design and to make, and not particularly that much money is involved. A design that proves a failure generally means, therefore, that much time is lost, because probably few parts of the old one can be used in the new one. I believe that the principle of the Hopkinson-Thring torque meter will provide a solution of both thrust and torque measuring difficulties.

Measurements of the *resistance of the aeroplane* can also be made by glides, preferably with the propeller stopped. A simple mechanism is used for holding the propeller stationary, consisting of a tube moving in guides and provided with a leather buffer which can be pushed against one blade—of course after stopping the propeller by shutting off the engine and stalling the machine. This method has only just come into use, but it will undoubtedly overcome many difficulties. It does not of course deal with one of the most important questions—the effect of the propeller on the resistance of the aeroplane. This uncertainty makes the reduction of partial climbs to a form in which they can be compared with model tests somewhat uncertain, although recent work suggests that the actual methods used were not so much in error as was supposed at one time. There is one objectionable feature of the gliding method—the loss of height at high speeds is very rapid, so that there is more likelihood of error due to variation in the state of the atmosphere.

There is, of course, a method for measuring the air force on any part of the aeroplane which is very attractive at first sight. It consists practically in inverting the ordinary wind channel and carrying the part along on the aeroplane attached to some kind of balance. For instance, we could attach the wings to a sliding frame moving more or less parallel to the chord. When applied to some problems this method may be of use, but for wings it has the great disadvantage that what is measured is the longitudinal force and not the drag, and the whole accuracy eventually depends on the measurement of angle of incidence. I shall refer later to an application of this method to measuring the position of the centre of pressure of the wings.

PRESSURE DISTRIBUTION.

I shall now turn to a different kind of experiment which is of the greatest importance in the comparison of model and full scale results—namely, the measurement of *air pressure distribution* on the planes and auxiliary surfaces. I believe work of this kind was attempted some time ago. At the R.A.E. much progress has been made in the development of it as applied particularly to wings. Bodies and tail planes and elevators have also been explored. It is very simple in theory. A number of small tubes are run through the wings and brought out at the desired

points and finished off flush with the surface. They are connected to a number of manometer tubes arranged so that the pressures can all be recorded photographically on bromide paper. By suitable arrangements the paper can be changed rapidly and many photographs can be taken in one flight. Fig. 5 shows the arrangement of tubes in a wing, Figs. 6 and 7 a photograph of the latest instrument, and Fig. 8 a typical record. It will be noticed that trunk tubes are run in the wing, equal in number to the manometer tubes available. Each trunk tube connects with a number of outlets to the surface, all of which, except the one to be tested, have to be sealed. It is the sealing of these holes and detection of leaks and stoppages which take most of the time in the experiment.

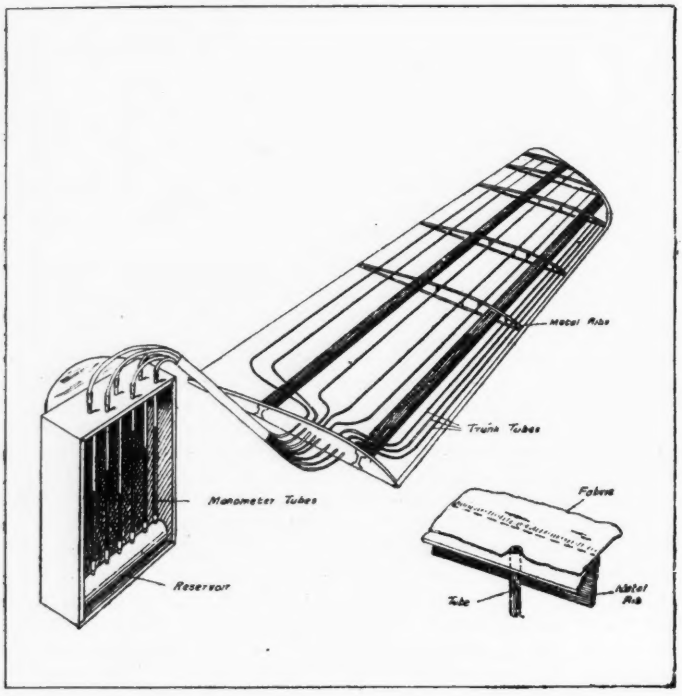


FIG. 5.—SKETCH OF ARRANGEMENT OF MANOMETER TUBES IN WING.

Very good accuracy can be obtained in the work. When it comes to comparing with models the difficulty arises that it is necessary to determine the attitude of the aeroplane, *i.e.*, the angle of incidence. Up to the present time the methods used for measuring angle of incidence have depended on the double measurement of rate of climb and inclination of the machine to gravity. The accuracy of this method is not as good as that of the pressure measurement. I think that the immediate future will see the application of a yawmeter arranged in a vertical plane and supported at such a distance in front of the aeroplane that the interference will be inappreciable. This could be connected to two of the manometer tubes and a record of angle of incidence thus obtained on the same paper as that of the pressures.

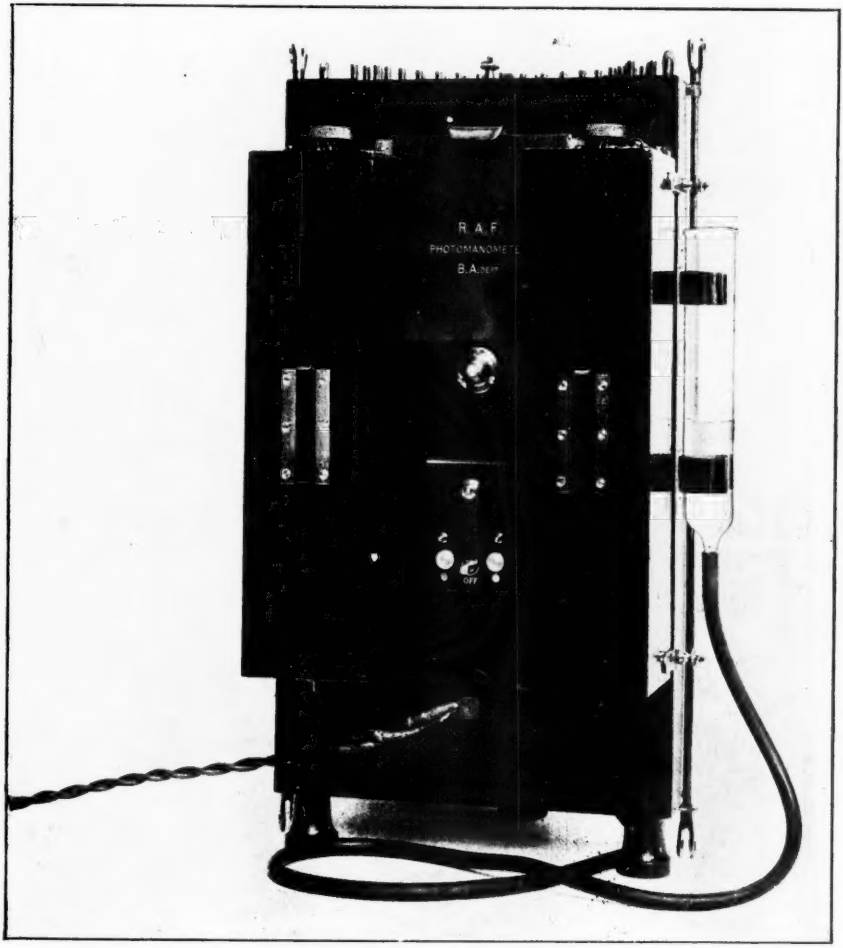


FIG. 6.—PHOTOMANOMETER.

Instrument closed ready for use. Note the two boxes, with inspection windows closed by brass slides, for carrying the rolls of bromide paper; large inspection door of red celluloid, closed by brass slide in centre; push switch for making exposure or lighting red inspection light; switch for selecting white or red lamp; reservoir for adjusting level of liquid in tubes.

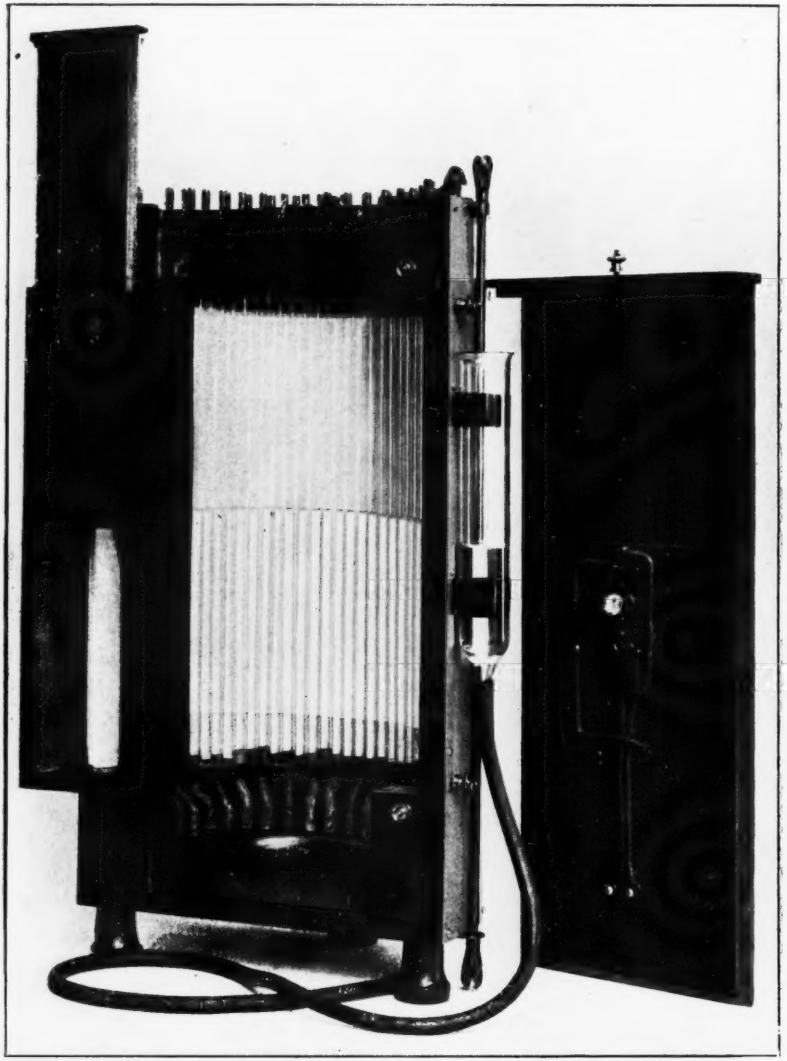


FIG. 7.—PHOTOMANOMETER.

Instrument dissected. Note the manometer tubes in this case arranged in a semi-circle connected to common reservoir; roll of bromide paper and paper passed behind tube ready for exposure; white lamp (for making exposure) and red lamp (for inspecting tubes in flight).

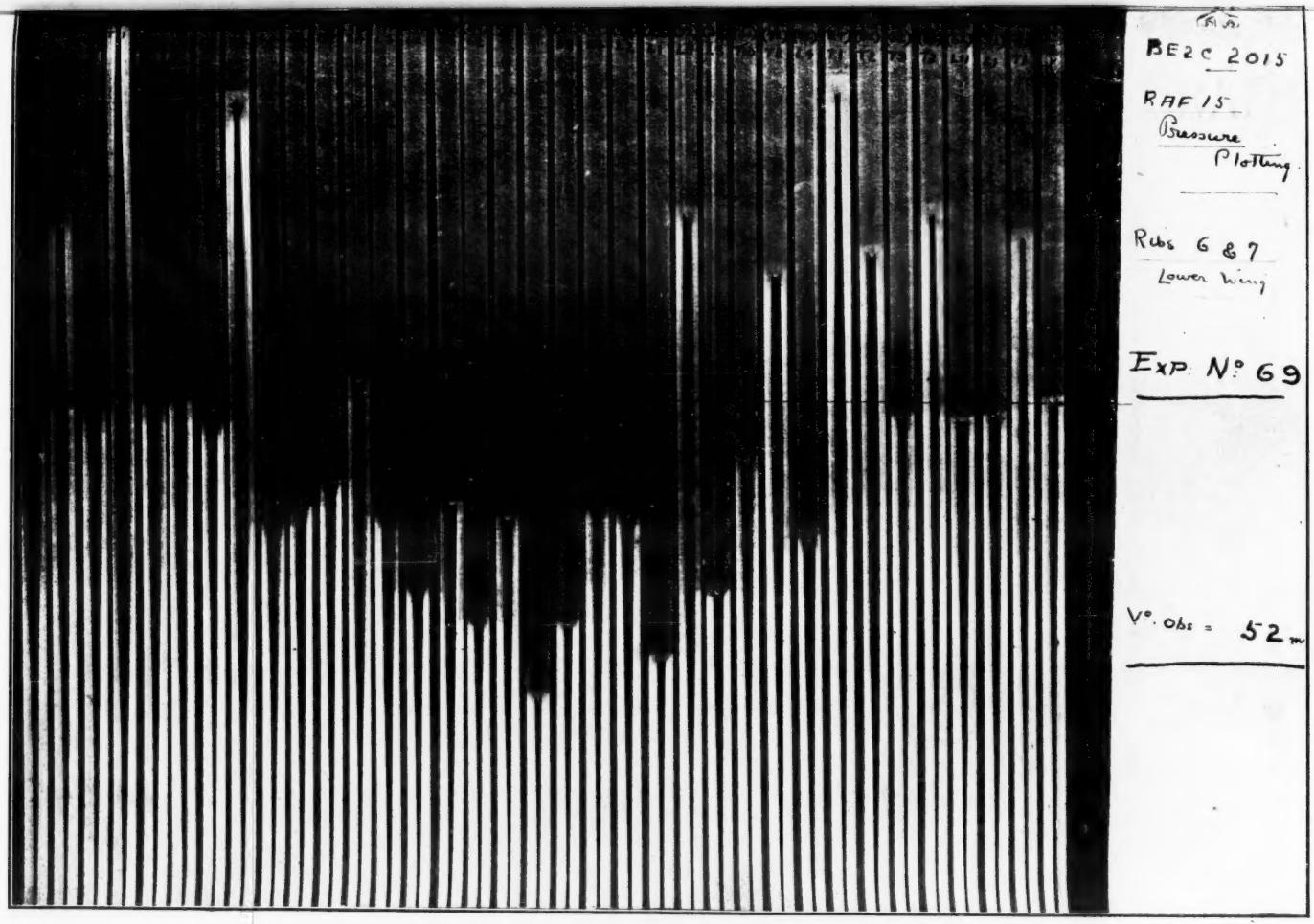


FIG. 8.—TYPICAL RECORD FROM PHOTOMANOMETER.

The lowest tube (centre) is connected to the pitot tube. Three static tubes (one next to the pitot tube and one at each end) enable the line of zero pressure to be determined. Note the very sharp image of the meniscus of the liquid.

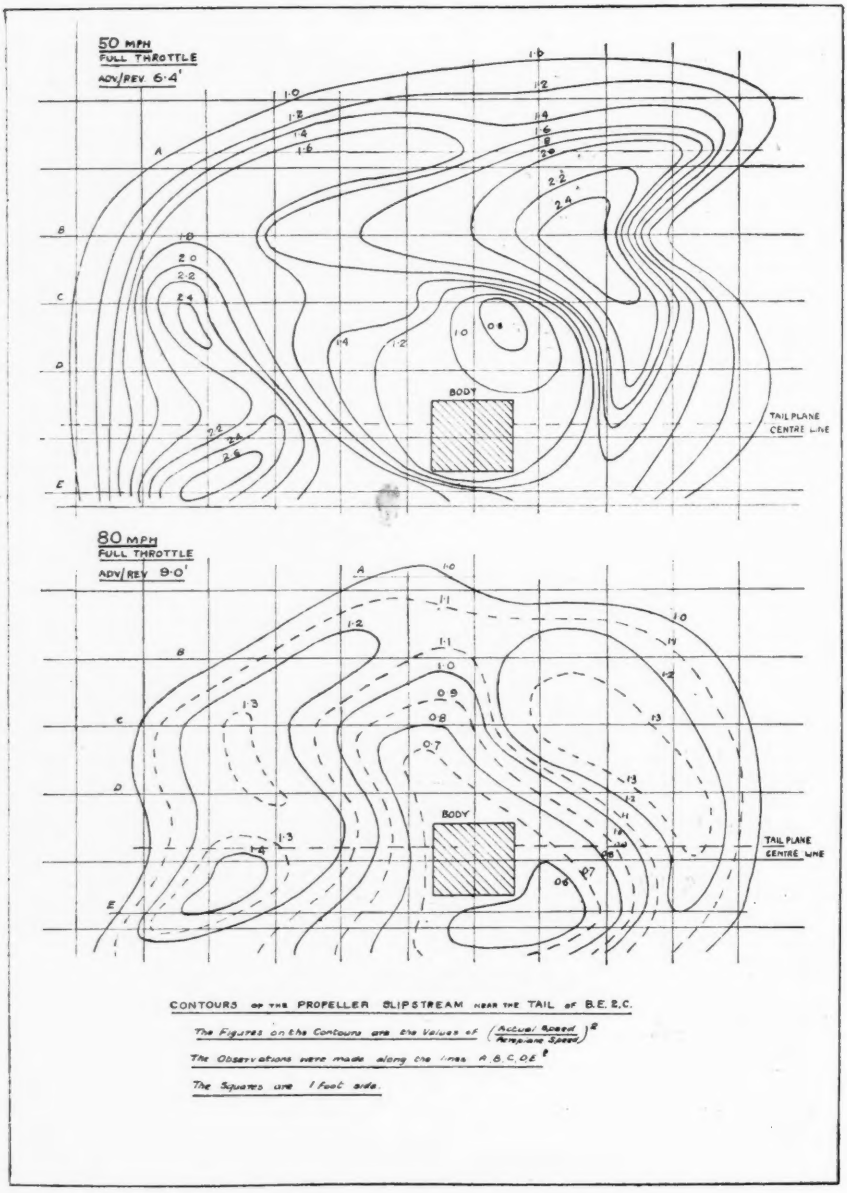


FIG. 9.—CONTOURS OF PROPELLER SLIPSTREAM NEAR THE TAIL OF B.E.2C.

Note how the slipstream is cut up by the wings, etc., into two main portions of high speed; also how the centre of the stream descends in relation to the body at high speeds.

Incidentally this suggests that the same apparatus, combined with a fore and aft gravity controlled level, both recording in the same paper, could be used to measure rate of climb or glide, and up and down currents would then be eliminated.

AIR SPEED EXPLORATION.

The method described for recording pressures has also been used successfully to measure the air pressure on other surfaces—tail planes, etc.—and on bodies. It is hoped to extend it to propellers in the near future. It has been used also in connection with pitot heads for the exploration of the air speed in front of the wings and near the tail plane. For this purpose it is very much more convenient than reading several dial instruments at short intervals, as such work is really beyond the power of an observer. It is for work of this nature that recording instruments are essential.

The air speed and direction at various points near the aeroplane have an important bearing on many parts of the design, particularly in connection with the tail surfaces, and with the propeller. This work is quite straightforward. I am unable to give details of results, as I could not devote enough time to the subject to do justice to it. Fig. 9 shows a very striking result obtained by this method. The peculiar and irregular shape of the lines of equal speed give an indication of the difficulties which face the designer when attempting to estimate the mean speed of the air over the tail plane, for example. This applies to many of the experiments which I shall mention briefly, and I hope the deficiency will be made up later on by others with more expereince of the details of the individual experiments.

CENTRE OF PRESSURE OF WINGS.

Pressure distribution results over the wings, if comprehensive enough, can of course be analysed to give the *centre of pressure* of the wings. A direct measurement of this quantity has been made by the following method, to which I referred briefly as an example of what one may term wind channel methods on the aeroplane in flight. An aeroplane was cut in two just behind the pilot's seat and the rear part of the body, with tail plane, etc., hinged to the front part by two ball bearings at the meeting points of the lower longerons. By a suitable arrangement of framework, wires and spring balance the bending moment on the body at this point could be determined directly. Fig. 10 shows the arrangement of the gear. As usual with experiments which seem very straightforward, the details of this one proved rather tantalising, and caused considerable delay. The reduction of the results is not simple because of the doubt as to how much of the air moment on the body itself is due to the part in front of the wings. However, the difficulties of both kinds were overcome and many arrangements of wings have been tested on this gear. I may say that it never gave any trouble whatever in flight as regards the control, etc., of the aeroplane. The possible angular movement of the rear part of the body was, of course, limited by stops and the gear was on the whole a good deal stronger than the aeroplane itself.

Up to this point I have dealt with experiments in which the main difficulties are in the actual measurement. I will now turn to a class of experiment where the chief trouble is to make up one's mind what to measure. I referred to these in the introduction to this paper as the most interesting kind of work we have to do. That is, of course, a matter of opinion, and I can only give my own.

STABILITY.

In reading through a section of the 1917 Report of the American National Advisory Committee on Aeronautics I came across the following sentence: "The most satisfactory basis for a single expression defining the degree of stability

is . . ." I will not say what the author's opinion was, because I do not agree with it. All I wish to say is that after three years' practical work on the subject I do not feel that I am in a position to complete that sentence wit has much confidence as did the writer of it.

In connection with fore and aft stability there are so far as I am aware only two measurements which can be made on a full scale aeroplane. A stable body when displaced from its position of equilibrium and released, oscillates about it with a steadily decreasing amplitude. We can measure the period of the oscillations of an aeroplane and the rate of decrease of their amplitude, in a number of ways. When we come to reduce the results we find that the accuracy of observa-

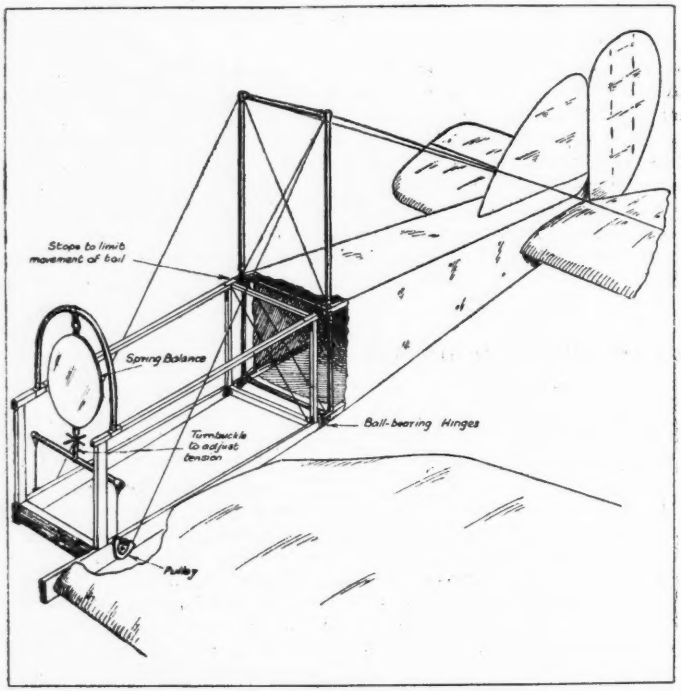


FIG. 10.—SKETCH OF APPARATUS FOR MEASURING LOAD ON TAIL IN FLIGHT AND HENCE CENTRE OF PRESSURE OF WINGS.

tion is hardly good enough to enable us to use them to determine the resistance derivatives. Moreover, in a moderately stable aeroplane the period is not sensitive —comparatively large changes in the aeroplane having small effect on the period and damping. The things that do seem to have a large effect on the period and damping are those which have generally been considered to be comparatively unimportant in stability calculations. The most important one is the propeller slip stream or rather its effect on the characteristics of the stabilising surfaces (this applies also to lateral stability). As a general rule, an aeroplane which has a comparatively long and ill-defined period of oscillation—say 30 seconds—and a fairly rapid damping with the engine on full throttle (I may say this is a common combination) will, when the engine is shut off and the aeroplane is trimmed for the same speed by adjusting the tail plane, have a shorter and very well defined

period—about 20 seconds—with very slow damping. Now if the length of period is taken as a guide it is more stable in the latter condition. If the damping is considered to be a better indication, the stability is better in the former condition. In my opinion the length of period is a more useful guide—at any rate the aeroplane is certainly nearer to instability in the first condition I described than in the second. In fact some aeroplanes are unstable with the engine on and stable, at the same trimming speed, with it off. See Fig. 10, which shows results from two aeroplanes of this type and one in which the change is reversed.

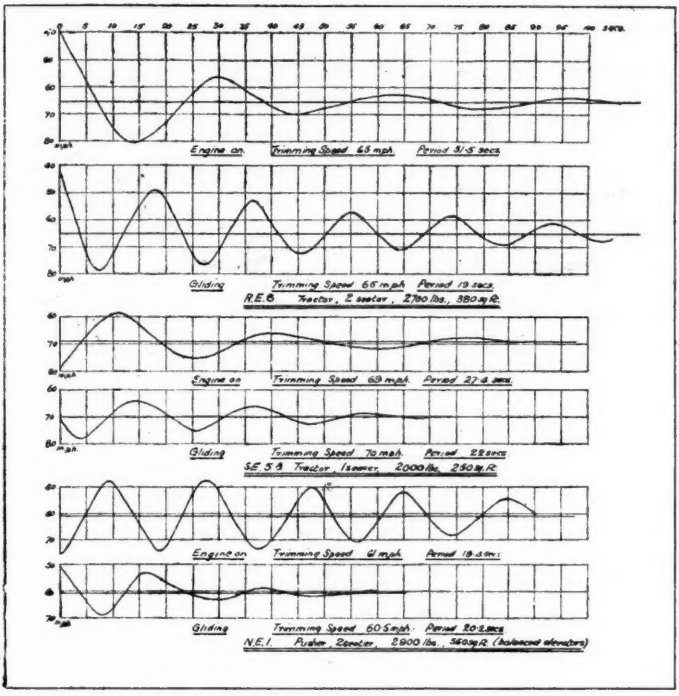

Engine on. Trimming Speed 65 m.p.h. Period 31·5 secs.
Gliding. Trimming Speed 65 m.p.h. Period 19 secs.
R.E.8 Tractor, 2 seater, 2700 lbs., 380 H.P.
Engine on. Trimming Speed 69 m.p.h. Period 27·4 secs.
Gliding. Trimming Speed 70 m.p.h. Period 22 secs.
S.E. 5 a Tractor, 1 seater, 2000 lbs., 250 H.P.
Engine on. Trimming Speed 61 m.p.h. Period 19·5 secs.
Gliding. Trimming Speed 60·5 m.p.h. Period 20·2 secs.
N.E.1. Pusher, 2 seater, 2900 lbs., 360 H.P. (balanced elevators)

FIG. 11.—TYPHICAL PHUGOIDS.
Illustrating difference between behaviour of machine with engine on and gliding.

I should like here to draw attention to what I think is a confusion of ideas in connection with the longitudinal stability of an aeroplane. The trimming speed of an aeroplane is the speed at which it will fly if the control column is released. For a given position of the centre of gravity this speed depends principally on the setting of the tail plane in relation to the wings, but also, in varying degree, on the engine speed, the total weight of the machine, and the air density. Keeping the tail setting unaltered, the magnitude of the effect of the change from "engine on" to "engine off" may be very large. The principal factors which determine this are also those which are most intimately connected with the stability of the aeroplane. But to comment on the stability of an aeroplane in terms of this change in trimming speed implies, in my opinion, a misconception of the nature of stability. I do not underrate the importance of making this change as small

as possible, but I think it should be realised that though the two matters depend on the same quantities, they are really quite distinct. The statement that an aeroplane is " nose-heavy with the engine off " may be true, but it is not useful to a designer as an opinion on the stability of his aeroplane.

The actual measurements of period, etc., can be made by recording, either continuously or at short intervals (one reading each 5 seconds is found to be easy to keep up with accuracy), almost any characteristic of the aeroplane—its speed, its angle to gravity, the engine revolutions, or the magnitude of apparent gravity. There is, by the way, one interesting fact in connection with the *direction* of apparent gravity. If there were no damping the direction of apparent gravity in relation to the aeroplane would not change—in the same way that a pendulum

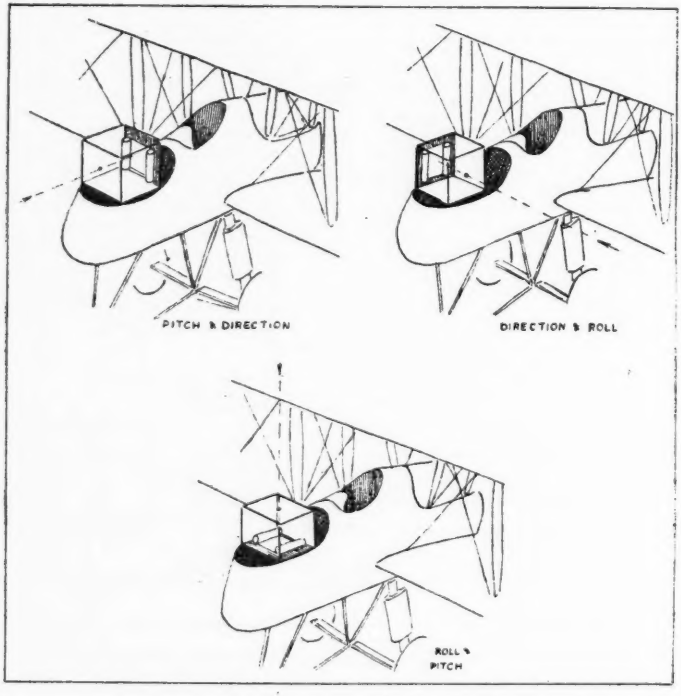


FIG. 12.—APPLICATION OF PINHOLE CAMERA WITH MOVING PAPER TO RECORDING VARIOUS MOTIONS OF AEROPLANE.

hanging in a truck moving without friction on an undulating track appears to an observer on the truck to remain stationary. This means that in practice a fore and aft level is unreliable as an accurate indicator of the attitude of the aeroplane if the aeroplane is—as of course it always is—oscillating with its natural period.

The most convenient method of recording the attitude of the machine is to trace the path of the shadow of a pin-hole cast by the sun on to a moving band of paper. This method can also be extended by using a form of pin-hole camera to measure the angular movement of the aeroplane in direction or in roll, or in both simultaneously. Fig. 12 shows diagramatically the arrangement in each case. To measure roll and pitch simultaneously, the sun would have to be vertically overhead, which rules out this combination. This method was originally due to

the late Dr. Lucas, to whom we owe a number of very important instruments and methods used in aeroplane experiment.

Lateral Stability is even more difficult to measure than fore and aft stability—in "lateral" I include what are colloquially known as "directional" and "lateral" stability. These are actually not separable in the sense that the longitudinal (*i.e.*, symmetrical) motion of an aeroplane is separable from asymmetrical motion. For a long time it was merely a matter of opinion—aeroplane A has "too much" or "not enough fin," and very often two men would give quite different judgments on the same point. Rough and ready methods were devised to give some idea of the relative stability of two machines, but it generally came down to opinion in the end. Recently the plotting of the angular motion of the machine has been developed with the aid of the instrument I have just described, but results have not yet been put into convenient form.

The necessity for devising some simple tests for getting at any rate a qualitative idea of the lateral stability of an aeroplane made itself felt in a rather curious way. A typical B.E. 2C was reported by both the pilots, who at the time formed the whole of the flying staff of the R.A.E., to have become suddenly and unaccountably uncontrollable laterally. They were both quite definite about it—the fact that for once they agreed in their opinions was in itself an indication that there was something seriously wrong. A close inspection of the machine revealed nothing abnormal—the fabric was tight, controls worked easily, the rigging was normal. Unfortunately neither of the two pilots could give much useful information to help to elucidate the mystery—they talked a language which we found it rather difficult to translate. The one piece of constructive criticism that they offered was that in their opinion the machine was almost unsafe. Short of burning the whole thing as the quickest way of ridding us of an evil spirit there seemed little to be done. However, in spite of its unsafeness neither pilot seemed to object particularly to flying it, so some flights were made with an observer to see if any information could be obtained in that way.

It was considered unlikely that the observer would be able to say anything about the controllability (*i.e.*, the effect on the machine of a definite movement of the controls), but he could at least record what the machine did when *left to itself*.

The scheme was as follows. Starting in each case from steady flight, and repeating each test at least once, and again with the engine shut off (gliding) :—

1. Release all controls.
2. Release all controls, kick rudder to right, and release.
3. Release all controls, kick rudder to left, and release.
4. Bank to right about 30°, keeping machine straight with rudder, and then release all controls.
5. Repeat with left bank.

The amusing part of the whole affair is that what happened when these manœuvres were carried out was so exciting that the lack of lateral control seemed to fade away. It was apparently simply bad temper on the port of the aeroplane, and the usual cure for this ailment—taking no notice of it—proved effectual.

Eventually many other machines, of every type we could beg, borrow or steal, were tested, and the tests repeated by other observers, and gradually we got together a fairly coherent set of results. The same people had to test each machine because the observations were simply descriptive, and the violence of the subsequent motion could only be estimated by reference to the impression left on one's mind by the previous experiments.

The results of these experiments, combined with the opinions of all the pilots whom we could get hold of, were used to form the basis of standards of lateral stability to which all subsequent R.A.E. aeroplanes were designed. They were vague enough, I must admit, but I feel they were better than purely arbitrary ones.

One thing which impressed itself very strongly on me from observing these experiments was the great effect of the asymmetry produced by the propeller and slipstream on the behaviour of the aeroplane. I do not think it is any exaggeration to say that half the difficulties in interpreting stability experiments are due to uncertainties in regard to the propeller and slipstream. It is on this account that so much work has been done on the latter subject in particular.

CONTROLLABILITY.

Controllability of an aeroplane is mixed up to a great extent with stability, and it has proved even more difficult to measure. One of the simplest measurements that can be made is that of rate of turn. It can easily be shown that there is a smallest circle in which a machine can be turned. If the controls are powerful enough the diameter of this circle does not depend on the size or speed of the machine. This is supported by experiment, though when we come to aeroplanes so large that the diameter of the smallest theoretical turning circle is approximately the same as a dimension of the machine, it will probably cease to be true. The wings of the aeroplane will give the most lift at stalling angle and the lift will be the weight of the aeroplane multiplied by the ratio of the squares of the actual speed and the stalling speed. If, then, the aeroplane is banked vertically this force will supply the necessary central acceleration, *i.e.*, the mass of the aeroplane multiplied by the square of the actual velocity and divided by the radius. Therefore the minimum radius is independent of the mass or the actual speed, and is given, in fact by (stalling speed in ft./sec.)^2 divided by the acceleration due to gravity. For a stalling speed of 40 m.p.h., say 60 ft./sec., the radius is about 110 feet. Many measurements of the smallest radius of turn which could be obtained have been made either by taking the time of a turn at a known speed, or by camera obscura observation. The maximum acceleration has also been measured directly. It was found that for a number of different aeroplanes the minimum radius obtainable was above the theoretical minimum but by an approximately constant percentage (20% to 30%).

This measurement is comparatively simple because the motion is fairly steady. When we come to *lateral and directional* control the motion cannot be kept up indefinitely, and not only does measurement become difficult but the actual motion of the aeroplane depends on the pilot and how rapidly he moves the controls to predetermined positions. For instance, the time to bank to 45° was measured for several aeroplanes and the time recorded was found to vary in apparently identical conditions on the same aeroplane with the same pilot by as much as 1sec. in about 3secs. By great care a very valuable set of results was eventually obtained, which did clearly put the machines in an order of excellence, but that order was not probably the order of preference of the average pilot of these machines.

I am not sure that we shall ever be able to put down everything about this very debated question in black and white—personal preferences will always have a good deal of weight. But we shall gradually clear up a number of uncertainties which now trouble us, and the time will come when we shall be able to say to a man, " That machine will do what you want, and it is better for that purpose than any other machine," and if he says he cannot make it do what he wants, we shall be able to say that the fault is his and not the machine's. At present we are inclined to hang our heads and go away and try to satisfy what are in many cases purely personal prejudices.

I should like, if time permitted, to go into more detail on this subject and to describe the latest developments: the use of the cinematograph; instruments for recording the movement of controls; devices for replacing the pilot's hand by a definite force or rate of movement of the control; the experimental investigation of spinning and other " stunts," one of the few places in which a fairly coherent

theory has been developed from full-scale experiments only; and the measurement of the stresses which are put on an aeroplane in various manœuvres. This last experiment is of such importance that I think a short description will be of interest, although I feel strongly that it deserves more complete treatment, which will, I hope, be given by those who have been responsible for the development of the instrument used. It was this instrument to which I referred in connection with the possible improvement of barographs.

The deflection of a loaded spring which is supported in an aeroplane by one end so that it can swing freely in any direction will measure, when the loaded end is stationary with regard to the aeroplane, the magnitude of apparent gravity in the machine—apparent gravity being the resultant of (1) the force per unit mass required to produce the actual acceleration of the aeroplane at the time, and (2) the force per unit mass of the attraction of the earth. These two forces generally have different directions, except in normal steady flight. If the direction of gravity in relation to the aeroplane is known, the direction and magnitude of the other force can be determined, and hence the actual acceleration of the aeroplane. But even if the attitude of the aeroplane is unknown the total force is always measured. If the deflection of the spring in normal steady flight is one inch, then under any other conditions the number of inches deflection measures directly the total air force on the aeroplane, the unit being its weight. If the spring is constrained to deflect in any given direction, then the force in that direction is measured.

A simple instrument of this kind has been used to a considerable extent, but for rapid manœuvres it is not practicable to make it of sufficiently short period to ensure accuracy. Dr. Lindemann, of the R.A.E., has devised a very beautiful instrument, compact and light, with a very short period. I will not describe it because that will, I hope, be his privilege on some future occasion. It records the component of apparent gravity in one direction (generally approximately perpendicular to the wing chord) on a travelling film, and recently an attachment for recording the air speed simultaneously has been added by Dr. Searle. By permission of the Controller of the Technical Department of the Department of Aircraft Production I am able to show three typical records, two of accelerations on a scout and a two-seater during a mock fight (Fig. 13) and one of acceleration during various stunts (Fig. 14). Many other experiments have been made, loops, rolls, spins, etc., being shown very beautifully.

This instrument not only gives us, in any circumstances, the resultant air force on the aeroplane—information which is essential for fixing a proper standard of strength—but it also measures the time of rapid manœuvres. It is no exaggeration to say that it is one of the most valuable instruments for aeroplane experiments—and, indeed, for any other experiments where rapidly changing accelerations have to be measured—that has been devised.

I have given you an outline of some of the most important experiments which have been carried out recently and I have indicated sometimes directions in which research is going. That question formed the subject of Dr. Durand's recent lecture and I shall not try to improve on his treatment of it. I wish to conclude with a short statement on the question of the organisation, equipment, and other difficulties of administration connected with full-scale aeroplane experiments.

ORGANISATION.

I mentioned earlier that only on about 35 per cent. of the days in the year are full-scale aeroplane experiments really possible. I cannot produce any statistics in support of this figure, which is simply my estimate based on experience. Assuming it is correct, and remembering that the *distribution* of the good days is

ACCELERATIONS ON SEs AND BRISTOL FIGHTER DURING A MOCK FIGHT 19 MARCH 1918.

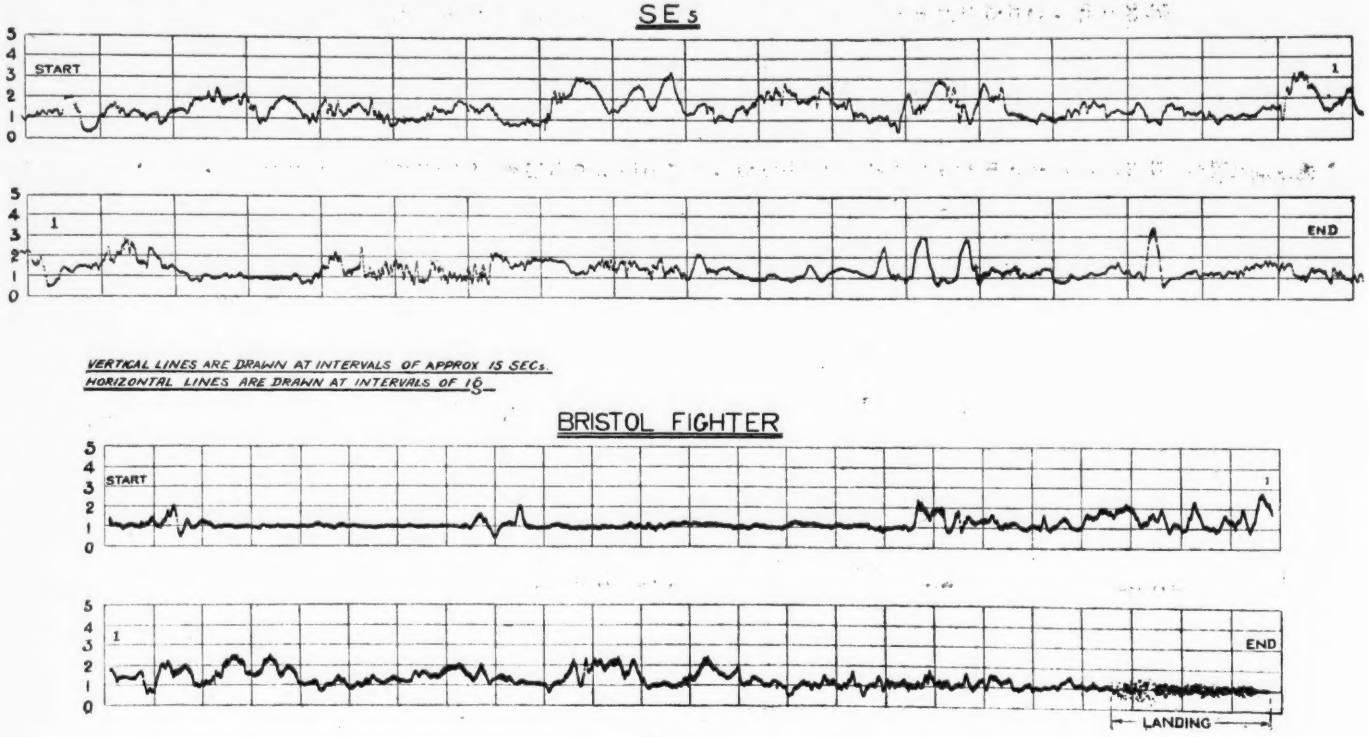


FIG. 13.

ACCELERATIONS & AIR SPEEDS DURING VARIOUS MANŒUVRES ON A BRISTOL FIGHTER

10TH MAY 1918

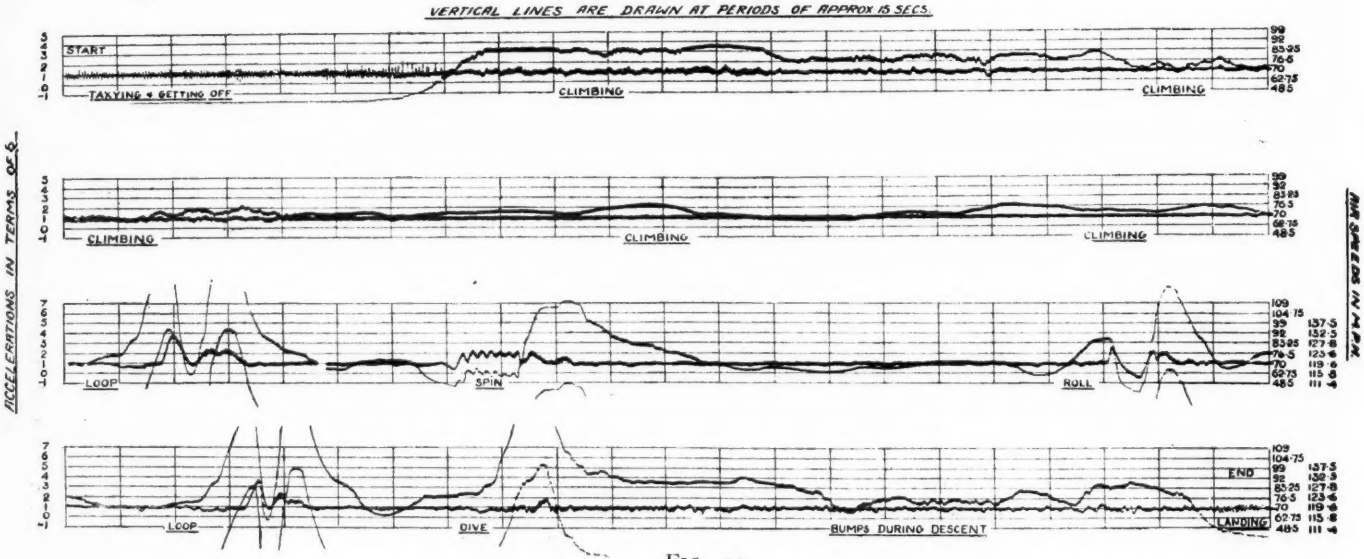


FIG. 14.

liable to be very erratic, it is evident that one of the chief cares of anyone responsible for experiments will be so to order things that no good day is wasted. One has always one safeguard. After a few days of bad weather, the weather often improves suddenly. Experience shows that the then apparently ideal weather is really untrustworthy. Once the weather has really settled at "bad," one is fairly certain of at least one, and generally two, days' notice of a change for the better, so that one can go ahead with repairs and alterations, taking up to that amount of time, with a fairly easy mind. But bigger jobs mean, unless luck happens to be with one, that some good days will be wasted for that machine. It all comes down to a simple matter; do not dock all your machines at once—get the repairs and alterations done quickly. It is the endeavour to do this which requires considerable organisation.

Experiments cannot be conducted economically unless a fair number of aeroplanes are available. This is partly the logical outcome of what I have just said, and partly due to the fact that the equipment which is necessary for running one machine will do, with hardly any expansion, for six.

Again, repairs and alterations to an aeroplane—which may involve almost every metal and wood-working process—cannot be carried out quickly unless there is a large plant and staff at hand. Here also what will do for one aeroplane will, with proper arrangements, do for several—in fact, the plant will not be always fully employed even then. The work will never be uniformly balanced. It has a way of overloading one kind of tool. We immediately find ourselves involved in orders of urgency, forms, conferences and all the routine and order so hateful to what describes itself as the truly scientific mind. In spite of all the arguments and the hard things that have been said, I am convinced that it is not possible to do without these hindrances and that the solution of the problem which has been arrived at more or less by trial and error is the right one.

I summarise the case as follows :—

1. You must not waste weather.
2. You must not waste men's time.
3. You must use men as you find them.

Therefore:

1. You must have a number of aeroplanes available.
2. You must have large resources for repair, construction of experimental apparatus, etc., immediately available.
3. Since you cannot find continual employment for all these, there must be some kind of work on which they can fill in their time and which they can, if necessary, *all* drop at a word. There must be no promises of what number will be produced; the less they produce the more reason will you have to say your experimental equipment is well balanced. You must not divide the total cost of running the whole establishment by the number of whatever it is that they produce, and complain that you can get them at one-tenth that amount elsewhere—the value of the articles produced should be regarded as a clear saving because you were—or should have been—prepared to spend what you have spent and see no production return at all.
4. To manage an establishment of this kind you require men of a special type. You want a works manager who hates the idea of producing ten things of the same kind—an artist, in fact. But when you *do* give him a job he must put all his energy into it.

5 You must not be too keen on getting things done very quickly or very cheaply. To run the whole concern you must find a man in whom you believe, and you must trust him.

6. You must get the best technical men for the work. My experience is that the men who are best qualified for it are impatient of the restraint imposed by forms and rules and organisation generally. To use them to the best advantage, give them what they want in the shape of a place where they can fiddle about by themselves, without disturbing your main shops. This will not cost much and it will make them happy. Men work best when they are happy.

PROCEEDINGS.

FOURTH MEETING, 54th SESSION.

The Fourth Meeting of the Session was held in the Hall of the Royal Society of Arts, London, on Wednesday, December 18th, 1918, Lieutenant-Colonel M. O'Gorman, C.B., presiding.

The Chairman said Captain Farren was going to deliver a lecture on "Full-Scale Aeroplane Experiments." The subject was an interesting one, and the treatment Captain Farren had given it was exceptionally interesting. It dealt with experiments on appliances unsuited for accurate experiments, but the lecturer was exceptionally well suited for dealing with the subject. A long and at times abstruse scientific discussion took place and lasted nearly two years before a Committee appointed *ad hoc* to inquire into the relation between full-scale and model experiments, because that relation was practically fundamental to rapid aeronautical progress. Captain Farren was one of the moving spirits in that matter. The queries he and those working with him raised led to a fundamental probing into the question of the interference between one part of an aeroplane with another part, thereby making an important subscription to the whole science by which model work was rendered useful to the full-scale workshop. For a time he (Colonel O'Gorman) was Chairman of that Committee, and he was followed by Professor Petavel, who was more competent than he to take charge of such a Committee. At the time he (Colonel O'Gorman) was busy with other demonstration work, but he remained on the Committee and followed the work with extreme interest. By the time they had finished their work the disagreeing parties had not only agreed, but had found out much new matter and had formed a high esteem for one another. He was sure Mr. Bairstow would agree with him as to the effect of that discussion upon him, and that Captain Farren would also admit what a feeling of mutual esteem the two sides came to and what satisfactory results were eventually arrived at. Captain Farren gave just a breath of suspicion of that long discussion, where he quoted Dr. Durand, an American member of the Society, who was President of the National Advisory Board in America. He said no *a priori* answer could be given to the question whether model results could be applied, without modification, to full scale, but he agreed that full-scale work had proved that they could, in effect, be applied. That was a proof that the model work was valuable, but derived its value, to some extent, from work done on the full scale, instead of the model results being applied to the full scale by *a priori* logical thinking. His was a safe and proper way of proceeding, and incidentally it proved that great weight attached to a large consensus of full-scale results, in spite of the unsuitability of the air conditions to accurate full-scale measurements. Those matters were interesting, in that they led one to feel that Captain Farren had given long and careful study to this matter of full-scale work. He was at the Royal Aircraft Factory for some years, and continued in the R.A.F. establishment. He was now connected with a large industrial enterprise, and was quite the right man to address the Society on this subject.

Captain W. S. Farren, M.B.E., expressed his thanks for having been permitted to illustrate the lecture with slides prepared in the Royal Aircraft Technical Department, and to his personal friends who, with others who had unfortunately since been killed, had done the greater part of the work he was going to speak about, and if he failed to do them justice he hoped they would forgive him. Any opinions he expressed were his own, and did not represent the official opinion of the Technical Department.

Captain Farren then delivered his lecture.

DISCUSSION.

Mr. L. BAIRSTOW, F.R.S., said many of those present were quite familiar with the work done at the Royal Aircraft Establishment, and could appreciate the labour involved before a lecture of the kind delivered this evening could be prepared. There were many representatives of the Establishment present, who were more acquainted with the more recent work, which was of a complex character, and he would leave to them the question of discussing the later data of the Aircraft Establishment. The difficulty of carrying out research work was well shown in the experimental production of an aeroplane. In dealing with a new type of aeroplane during the war it might be said that some 12 months elapsed between the date of the inception of it and the late of production in quantity. Three months were required for the production of the first two or three aeroplanes, which were made by hand, the whole of the arrangements, compared with the final production arrangements, being crude and primitive. On the other hand, the aeroplane maker realised that he must get the first aeroplane into reasonably good condition before he started to produce the machines on the final basis. After getting into production it would cost as much to produce 1,000 bad aeroplanes as 1,000 good ones. Hence the necessity for careful preparation. When the main details of design were fixed another three months' preparation of jigs, etc., followed. The fact that it took all this time to put one's knowledge into concrete form in a particular aeroplane gave some indication of the length of time necessary to compile the design data. England started to collect data in 1909, and for perhaps 12 months the work was almost entirely confined to wing sections. Contemporaneously work was going on in the improvement of engines. At a later stage Mr. Busk, who gave his life in the cause of aeroplane development, took up the application of aerodynamics to the stability of aeroplanes, but when the war came along, and fighting was the chief use of the machines, more attention was paid to controllability than to stability. Captain Farren's lecture showed how, with the development of aeroplanes, the development of research had also gone ahead. He expected that in the future the problem of stability and controllability would take on a new aspect, having in view the needs of civil transport, and that attention would again be turned to the development of satisfactorily stable machines. It had not been considered of primary importance in warfare. With regard to the organisation of research work, just as the first production of an aeroplane needed separate shops and separate methods of treatment, so research—which was the fundamental basis of all design—should be considered as a subject not to be dealt with by the ordinary routine of shop production. There was nothing more disheartening or detrimental to the organisation of a research establishment. He hoped that in future due consideration would be given to the necessities of research, as organised experiment must ultimately be more economical to the industry than disorganised and haphazard experiment.

Captain F. M. GREEN, R.A.F., said he had had the pleasure of working under the present Chairman nearly all the time he was at Farnborough—about seven years. The research work was started in a very humble way. He remembered when the first aeroplane came to Farnborough. He tried to persuade the pilots to make the simplest sort of speed measurements, but the pilots thought it undignified to make any measurements, and that their opinions as to what the speed ought to be was good enough. They only flew about 10 minutes a day on fine days. However, they marked out a course of a few hundred yards and by means of stop-watches and flags recorded 36 miles per hour. The first direct measurements of air speed were made with an old-fashioned anemometer. This was fixed on to the aeroplane and stopped and started against a stop-watch with a piece of string. He believed that was the first attempt at recording speed on an aeroplane directly, but since then things had improved and developed along the lines Captain Farren had indicated. It would be impossible for any private

manufacturer to undertake research on full-scale work in the way it ought to be carried out, and it was a national duty that it should be done. It was terribly expensive, and the amount of time wasted was almost unbelievable. It took a high class of investigator—men one could not afford to employ unless they worked for the good of their country or unless the employer had almost unlimited funds. He hoped the research side of the science would be encouraged, and that it would be supported loyally by all the manufacturers of aircraft and the public generally.

Major G. I. TAYLOR said with regard to pressure distribution measurements they had expected heaps of difficulties, but they did not experience any of them. The chief trouble was the difficulty when one had got the measurements and pressures all over the surface of a wing of getting any proper comparison with the model. Another difficulty was that if one wanted to use these experiments for integrating up and finding the total resistance of the aeroplane it was necessary to have so many holes all over the wing that it became practically impossible. However, he thought something was got out of it. The results of the pressure plotting agreed pretty well with the pressure plotting on the models, as far as he remembered. He thought it was the end of 1915 when they did this, and he had rather forgotten the details.

Lieutenant-Colonel CAVE-BROWNE-CAVE: I have been particularly interested by the description of the arrangements made for determining the speed of the aeroplane, and more particularly by the method of flying a previously calibrated machine alongside the one to be tested.

It would be of considerable value to know how far the accuracy of the speed of the calibrated machine may be depended upon. I know very little of the extent to which the performance of a machine will vary with the condition of the fabric in dry or moist atmosphere or with the very slight variation in the accuracy with which the machine is trued up.

The constancy of the performance of this reference machine has presumably been determined by a number of runs over one or other of the speed courses.

If we could know the accuracy with which the observed speeds over this course agree with each other we should be able to form some idea of the accuracy with which the machine can be relied on as a standard of reference and also of the accuracy with which the speed of the machine over the course can be determined by the observers.

Dr. F. A. LINDEMAN said he well remembered the difficulty Captain Farren often had to get measurements out of the pilots, and he was glad he had emphasised the importance of getting measurements of controllability and other quantities, rather than mere expressions of opinion as to the "feel" of the machine. Such vague expressions were not accepted in physics or other science, but definite figures were demanded. Until controllabilities could be measured there was no prospect of scientific advance.

The CHAIRMAN said he did not know how far those present realised that the speakers, who had so far only made brief contributions to the discussion, had done phenomenal experiments. They said that they had little to add to the matter, but they had done phenomenal things and deserved a Victoria Cross, if it were awarded for scientific work at the risk of one's own person.

Major B. MELVILLE JONES said it was about 2½ years since he did some of the experiments described in the Paper, but he had been connected with experiments in controllability in fighting machines. The Camel was more manœuvrable than a Bristol fighter, and yet he had known a pilot in a DH4 outmanœuvre the Camel. The DH4 man was exceptionally good. It showed how difficult it was to get any idea of controllability from any few observations. It was frightfully

difficult to get information as to controllability from direct scientific measurement, because all the best pilots get their best results by spinning turns and curious reversals, which were terrible things to follow by any mathematical process. Those remarkable results were more often obtained in manœuvrability experiments than in any other experiments. One had to be careful not to take too scientific a standpoint. When two pilots reported a BE2C, as Captain Farren said, it might usually be taken as a fact that something had gone wrong, and if it could not be found out by observation the observation was not good enough. A lot of research work might be sterile through being too highly organised. If asked for advice as to how to organise, he would quote Mr. Punch's advice to those about to marry—"Don't." There must be a little organisation, but organisation in relation to research was a good servant, but a bad master. There should be only the least organisation that was necessary, to start with. As soon as a large organisation was laid out on what were thought perfect lines a new type of experiments would be started which would break the organisation or the organisation would break it.

Lieutenant McKinnon Wood: Full-scale experiments are required for the two purposes of ascertaining whether model experiments can be applied to full-scale aeroplanes and to what extent, and of doing that part of aeronautical research which cannot be done on the model, which embraces the greater part of the investigation of stability and controllability. If information can be obtained from wind-channel experiments on models it can clearly be obtained incomparably more economically in money and in time, and may mean the saving of lives and property. A great deal of design has been based on model results, and often with great success. It is very important to investigate thoroughly the extent to which model results can be applied. Moreover, if model results are not misleading (and I think it is established that they are not wholly so), they can be used to suggest the lines for full-scale work and to select from a number of possibilities the most hopeful developments for full-scale investigation. For what I call experimental analysis the model has many advantages over the full scale. It is perfectly easy in a wind-channel to dissect a model—to fix one part and measure forces on the other part in its presence. We can in some cases do this in the air on a full-scale machine. An example quoted by Captain Farren is the aeroplane with its body broken and hinged behind the pilot's cockpit in order to measure the moment about this point of the air forces on the aeroplane. Another example is pressure plotting over a surface—the wings or tail. But experimental analysis is obviously easier and often only possible on the model. The model is also useful for testing theories, because the model can be made to represent the simple theoretical case, which may be concerned with a single wing only, whereas we cannot send into the air less than a complete aeroplane, which is a very complex affair. It is this very complexity which introduces a large part of the difficulties and uncertainties of full-scale experiments.

It is therefore very important to establish the applicability and limitations of model work and to make good use of models; we must not build too much on them till we have done this. In order to obtain a direct comparison between full scale and model it is almost essential to eliminate the engine. The lift and drag of a complete machine can be obtained by gliding with the propeller stopped, and this can be compared with the lift and drag of a carefully made complete model. The only source of error is up and down currents in the full-scale experiments. By taking a sufficiently large number of glides this should be eliminated. Another direct scale effect comparison can be obtained by carrying out with the propeller stopped the experiment already referred to, in which the body is broken and hinged. This can be repeated completely on the model. Pressure plotting over the wings, etc., can also be repeated on the model and a direct comparison obtained.

May I give one piece of evidence that model results are applicable to full-scale? Two machines differing only in the wing arrangement were designed, one of which came up to expectations in speed and climb and the other failed rather badly in performance. Models were subsequently tested, and it was found that the failure in performance of the one machine could have been predicted from the model tests. Other examples could be quoted.

As regards the use of full-scale experiments for doing what cannot be done on the model, most of the investigation of stability and control—that elusive thing the " feel " of an aeroplane—comes under this category. In general, in a wind-channel the model is only allowed certain degrees of freedom, in order to measure certain forces. Wind-channel experiments do not lend themselves to reproducing an unconstrained motion, such as occurs in flight, although they facilitate analysis of stability and control. I see no reason why the model method employed by Mr. Lanchester for investigating stability by means of gliding models should not be further extended. The experiment will, of course, require great elaboration if the effect of propeller thrust, torque and slip-stream are to be reproduced. As regards controllability, while the model can show that one form of aileron, rudder, or elevator will give a lighter control than another form, that is about as far as models will take us. Full-scale experiments are clearly essential. The investigation is a very complicated one. In one case at the R.A.E. during some rough controllability experiments one pilot reported one set of ailerons the lighter and another pilot favoured another set. Measurements of the time taken to bank 45 deg. using the rudder in such a way as to avoid side-slipping and of the force on the stick were taken by each pilot, and the measurements of each supported his opinion. The conclusion to be drawn from this is that each pilot made a different use of his controls. Two things follow: firstly, different arrangements of control are required for different pilots; secondly, for further experiments steps must be taken to ensure that a standard procedure is followed, which may require the use of virtually an automatic pilot. A part of this automatic pilot which has been in use is a gear for applying a constant force to the control stick or rudder bar for as long as required. The apparatus required for thorough investigation of control is very elaborate. The cinema has been used for measuring the path of the aeroplane and the roll, pitch and yaw; also the photokymograph due to Dr. Lucas described by Captain Farren; and the gyro can obviously also be used, and may provide the best apparatus for this purpose. Instruments are also required to measure and read the positions of all the controls and the forces applied to them. I will not go into details of these, as Mr. Stevens is here to-night and is much better able to deal with these than I.

Mr. Stevens said it was very difficult to measure controllability. He showed on the screen apparatus which measured the movements of a machine in the air, the positions of the controls, and the forces applied to them. It could be applied to elevators, rudders, or flaps, and by its use one could measure what a pilot did in any manœuvre. He also showed a method in which an aeroplane carrying a cinema camera followed and photographed the manœuvres of another aeroplane. They had also a spherical calculator, which would calculate in five minutes the roll, bank, and pitch of the observed machine—a piece of spherical trigonometry which would take a skilled mathematician about 20 minutes to do.

A vote of thanks, moved by the Chairman, was accorded Captain Farren, whose reply to the discussion will be communicated in writing for inclusion in the Society's JOURNAL.

COMMUNICATED.

Major SOUTHWELL: As it is only recently that I have had first-hand experience of full-scale experimental work, my attitude to this lecture must necessarily be one of congratulation rather than criticism. My congratulations, however, can

be all the more sincere in that it has been my privilege to take over the general control of this work from the point at which Captain Farren left it, and I therefore know the high degree of efficiency to which the work of the Experimental Staff of the Royal Aircraft Establishment had been brought under his guidance. One or two points have occurred to me when hearing and reading his lecture, which I shall take in the order in which they were dealt with there.

(1) *Suitability of weather for flying.*—Captain Farren suggests that in order to obtain the best results work should probably be limited to about 35 per cent. of the days in the year. I am not sure (and at the R.A.E. I have found opinion to be fairly evenly divided on the question) that on the whole better results will not be obtained by flying in all weather which he would consider "suitable for test work," thereby increasing very largely the number of repeat observations which can be made within a given period. Of course, in some cases nothing but the highest possible accuracy is of any use at all, and in all cases it would be desirable to indicate on the diagrams those points which have been obtained from observations made under weather conditions regarded as "suspect," additional weight being given to results obtained on good days. I think this procedure would have a practical advantage in that it tends to keep the interest of pilots and others more alive during the whole course of an experiment; again, experience has shown that accurate repeat observations have sometimes been made on days when the weather conditions had been regarded with suspicion. My main point is this: above all things, it is important to avoid waste of good flying weather, and I believe this result would be best achieved by flying as often as possible, but making suitable allowance for possible errors suspected to be due to up-and-down currents, etc.

(2) *Recording v. non-recording instruments.*—Captain Farren sums up on the whole in favour of a simple instrument and good observer, as against the recording instrument. There is a third method now coming into use at the R.A.E., which I am inclined to think is preferable to either of the above, where simultaneous readings of several quantities have to be taken. It consists in the employment of non-recording instruments in conjunction with an accurate chronometer, placed close together and photographed (all on the same film) at approximately equal intervals of time. Except where it is essential that *continuous* records shall be kept, it is obviously wasteful to have each separate instrument fitted with a time-recording device; moreover, it is easier to make accurate measurements of time intervals in the way described than to construct an apparatus which shall make and break an electrical circuit at accurately equal intervals. It is a great advantage, too, when employing a new type of instrument to be able to use it at once in the form in which it is received (it will almost always be without recording apparatus) instead of having to modify it so as to make it record. The advantage which Captain Farren mentions, that an observer is able to note anything abnormal which occurs during a flight, is intensified in this method, since his attention will not be distracted by the necessity of taking measurements; and since in reducing observations it is always necessary to deal with simultaneous values of the different quantities at a *finite* number of instants, the data is obtained in exactly the form required.

(3) *Organisation.*—I am glad to see that Captain Farren has stated his conviction of the necessity for a certain amount of routine and organisation, for the evil results of too much system are so obvious that it is an act of some courage to pronounce in favour of any at all. From the remarks of some speakers after the lecture one would conclude that the best results are obtained by freeing investigators from all control and leaving them to work quite independently on their own experiments. This view I believe to be perfectly correct as regards most types of laboratory experimental work; but the peculiar characteristic of full-scale research is the very large number of people who must

share in the work of any given experiment, and many of whom must, from the nature of their work, conform to some sort of routine; and for this reason I believe that a policy in which only the requirements of the individual scientist are considered will not be justified by its results. The completely uncontrolled investigator no doubt obtains his results rather quicker than he would if obliged to conform with a certain amount of system, but somebody has to clear up after him, and when his activities bring him into contact with shops, drawing office, and the personnel of the running sheds, in addition to the staff engaged on the reduction of experiments, this clearing up may involve an immense amount of unproductive labour. After all, full-scale research is a big undertaking, and it would seem only fair that the scientist who engages in it should be ready to put up with the same amount of inconvenience, in order to ensure smooth relations with the other craftsmen concerned, as the pilot who is asked to take accurate and methodical observations during evolutions which in themselves impose considerable strain upon him.

Mr. STEVENS: In connection with Captain Farren's lecture on full-scale experiments before the Royal Aëronautical Society, I should like to raise the following points:—

(1) In discussing the measurement of speed by indicators on the machine, he conveys the impression that most of the error (2 or 3 m.p.h.) is due to the air striking the pitot head at an angle and can be eliminted by wind-vanes on a swivelling head which keep it truly up wind. This is not the case, as in the experiments he quotes the error was only very slightly reduced by allowing the heads to swivel, but was brought down to ½ per cent. by mounting these swivelling heads some 8ft. ahead of the wing structure out of the region of interference.

(2) Under the same heading he mentions differential liquid gauges, and states that these work on the equivalent of a liquid of a density equal to the difference of the two liquids employed. I am not aware of any instrument working on this principle and am of the opinion that such an instrument is impossible. In the differential gauges used at the R.A.E. two liquids of nearly equal density are certainly used. They are in a U-tube; one is coloured, and their surface of separation is used as indicator. Their free surfaces are in extensions of the U-tube of considerably larger bore. Consequently the motion of the liquid in the small parts of the U-tube is much faster than the motion of the free surfaces. Thus the surface of separation has a large motion and magnification is thus secured. The liquids have to be of slightly different density to provide a good surface of separation, and it is true that the greater this difference the less the sensitivity of the instrument. But even if the densities were exactly equal, the magnification would only be $\frac{1}{2}\frac{\text{area of large tube}}{\text{area of small tube}}$, and it is on this difference in area that the instrument depends.

(3) In considering the measurement of aeroplane resistance by the gliding method, Captain Farren says that one objectionable feature is the rapid loss of height at high speeds, so that there is more likelihood of error due to variation in the state of the atmosphere. In my opinion the larger the range of height in an experiment and the greater the distance travelled, the more likely are the effects of up-and-down currents, our chief enemies, to be eliminated. Glides are usually carried out over the same range of height, about 7,000ft. drop at all speeds, and, provided none are so short that timing and lag errors are appreciable (that is, less than about two minutes), they should all give equally good results. This is borne out by experiment.

In conclusion, I should like to say that I worked under Captain Farren some three years, and agree almost entirely with his remarks, with the exception of the points I have mentioned.

Captain FARREN: I agree entirely with Captain Green in his opinion that it is a national duty to undertake research on full-scale aeroplanes. I think it is very important that the work should be pressed on at once and every endeavour made to avoid a temporary slackening.

I think Colonel Cave misunderstood my reference to "formation" speed measuring. This is merely a direct comparison of the indicated speed of the machines, and is independent of any absolute measurement. One machine, however, being previously or subsequently calibrated by an absolute method, the test serves to calibrate the others.

Colonel Jones emphasised quite rightly the fact that where there is smoke there is generally fire—when an average normal man says there is something wrong with an aeroplane there is a certain definite probability that there is ground for his remark. I agree with this opinion; but in regard to the case to which I referred I was careful to ascertain subsequently that no definite explanation was ever given, nor was any definite remedy found—the peculiarity gradually disappeared.

It is possible that I did not make it quite clear in my remarks on the type of organisation necessary, that I referred only to the organisation of the *service* branch of an experimental establishment. I agree entirely that an elaborate detail organisation of the research itself is a distinct hindrance on account of the loss in adaptability which it incurs. My only purpose in referring to this subject was to take the opportunity to express an opinion in favour of a *large* service establishment. From my experience I think that anything else will be uneconomical in weather, men, and money, and will definitely retard research.

Lieutenant McKinnon Wood gave a very important piece of information in connection with the difference of opinions given by pilots on the question of control, etc.—that in one particular case, where A said that machine X was lighter on controls than Y, and B said the opposite, measurement proved that both were right; they *used* the machines differently. Colonel Jones's reference to D.H. and B.F. pilots outmanœuvring Camels with their fixed gun bears on the same point. Both speakers, I think, agree with my statement at the bottom of page 43—personal preferences will never be entirely eliminated, at any rate in fighting aeroplanes.

Mr. Stevens's contribution was very valuable, and serves to show how much the work is progressing.

Mr. Stevens has drawn my attention to an omission on page 58 in connection with speed course factors. The swivelling pitot head, with which a nearly constant factor of about 0.5 per cent. is obtained, is placed some 8ft. in front of the wings. The reduction in the discrepancy is therefore probably due more to the reduced interference generally than to the elimination of the error due to yawing of the head. This is more in accordance with expectation—angles of yaw up to about 10 deg. produce comparatively small errors in the readings of the standard pressure head.

I should like to draw attention to the fact that five out of the eight speakers made reference to controlability. This is undoubtedly the subject about which we know least, the most difficult one to experiment upon, and almost the most important.

I am obliged to Major Southwell for his kind remarks about me. In reply to the points he raises:—

(1) There is, I know, considerable difference of opinion on this matter. Personally, I adhere to the view expressed in my paper—in favour of restricting experiments (of certain classes) to the best weather only. It is very difficult to

decide *how much* weight to give to a result obtained on a bad day. By confining one's attention to deciding whether the day is good enough or not—a considerable task—and then accepting all the observation on good days and giving them all equal weight, the work is simplified and, I believe, the accuracy is improved.

(2) This method was considered by me many times. I did not use it principally on account of the instrumental difficulties. But I do not agree that the observer is in a better position, with this method, to note anything abnormal. In fact, it hardly differs in this respect from an automatic recording instrument. The whole point of the method which I prefer is that the observer *has to read the instruments*—it is only by the abnormality in their reading that his attention attention can be drawn to what is occurring—*e.g.*, a *sudden* change in rate of climb (which an experienced observer can easily detect as he makes his reading, but not in any other way); cause, up or down currents; remedy, stop the climb, as time is being wasted. This is only an illustration. I realise quite well that these are circumstances in which this method is inconvenient, and I am alive to the advantages of the recording instrument, including the type referred to by Major Southwell.

(3) Organisation is, I believe, absolutely necessary in full-scale aeroplane experiments—I mean organisation of the establishment, considered as the *servant* of the experimenter and not as his master. It is only the failure to observe this distinction that has brought " organisation " into disrepute.

I fear I did not sufficiently express my acknowledgments to the Staff whom I had the honour to serve during the three years I was at Farnborough. My paper is largely a summary of the work they did. My only regret is that I had to make the account so impersonal.

ABSTRACTS.

Power Curve for the Aerial Propeller.

The author employs the method of least squares in order to determine the actual law governing the relation between the power of the airscrew and the ratio V/ND, or forward speed/revs. per min. x diameter. He uses model results from Eiffel, and corresponding full-scale figures from Captain Dorand. He shows that these results are more nearly represented by a formula in which P, the power, is made to vary as $N^{3.076}D^{5.15}$ for a constant V/ND, instead of varying as N^3D^5 as accepted theory requires. The practical utility of any such formula is open to doubt, especially as it appears to assume an accuracy in the experimental data which is probably unwarranted. ("Aerial Age Weekly," November 18, 1918.)

Stresses in Aeroplane Ribs.

In a paper presented at a meeting of the American Society for Testing Materials, Irving H. Cowdrey, gives a method of transverse testing under non-uniformly distributed load with special application to the wing ribs of aeroplanes.

At the outset it is pointed out that many of the members entering into the construction of the framework of the aeroplane are of such form or construction that it is very difficult, if not impossible, to calculate the stress distribution in and between the different parts of the members. This condition has led to some attempts to test certain of the completed portions of the craft, but the results of the tests have usually been of doubtful value, owing to the conditions of test differing very much from actual conditions.

In the method described in the paper, a wing rib is tested under any desired distribution of loading by applying the loading on the rib through elastic rubber bands of different widths. A wing rib with short lengths of front and rear spars attached is supported in the upside down position from two horizontal bolts, each of which passes through the centre of the spar section. The rubber bands, which were cut from a motor cycle tyre, are attached at their upper ends to a series of stirrups which rest on the rib at intervals along its length; the lower ends of the band are connected to a stiff horizontal beam of wood, to which is applied the weight constituting the total load on the rib. Each of the distributed loads acts on the upper side of the rib under test (which is the under side in flight). In the example given of an actual test, the total load was divided into 14 parts, which number, it is stated, could well be increased. The deflection of the rib was measured at various points along its length, and a diagram of deflections is given for various total loads. The load distribution, as calculated from the widths of the rubber bands and the points of application of the forces was checked from the observed extension of each band and its calibration. There was a slight difference due to distortion of the rib. ("Aviation," November 1, 1918.)

Properties of Aeroplane Fabrics.

In March, 1917, the U.S. Bureau of Standards issued instructions for the manufacture of cotton fabric for experimental purposes, and in August, 1917, this fabric was adopted, and is known as Grade A fabric. The properties required by doped and undoped fabrics are described, also the various tests of the apparatus to carry them out, such as an inclination balance for testing tensile properties of textile material, and an apparatus for testing the bursting strength of cloth under uniformly distributed pressure. The inclination of balance is described by means of a diagrammatic drawing, and there is an arrangement for taking an autograph record. Several examples of such diagrams are given which clearly

show the yield point and the various characteristics of the fabric; these diagrams are discussed at some length. In the apparatus for testing the bursting strength, the fabric is placed in a frame over a sheet of indiarubber to make it airtight, and the air pressure is then applied. (E. D. Walen, "Journal of the American Society of Mechanical Engineers," November, 1918.)

Parachutes for Aeroplanes.

From various reports it appears that the fitting of parachutes in case of need is endorsed by experienced pilots who have actually tried parachute drops from aeroplanes. Experiments have been carried out since 1916, but it cannot be stated if any service planes have been fitted with emergency parachutes. Various reports point to the fact that parachutes now exist which, instead of being pulled out below the aeroplane by the pilot jumping out, can be launched upwards and so pull him out above the machine, which is left to fall to earth below him. Such a type known as the "soaring parachute" is necessary in the case of a machine in a flatspin with the controls carried away of a machine wtih the wings shot off, otherwise the machine might spin into the parachute in the one case, or even fall faster than the man in the other. Thus there are limitations to the type which depends upon gravity for releasing the apparatus. ("Air Service Journal," September 26, 1918.)

Use of Oxygen Tanks on Aeroplanes.

This is a short note on the advantages derived from the use of automatic oxygen apparatus in flights for any length of time at an altitude of above 10,000 or 12,000ft. The few who are able to continue for any length of time beyond 10,000 or 12,000ft. have a sense of lack of air and open their mouths in breathing and breathe more quickly and deeply. Although they may feel perfectly fit and well, they are not as efficient as when near the ground. Reaction becomes slower, resulting in longer time taken to judge distances, to aim, etc., so that the pilot without oxygen is at a great disadvantage as compared with the pilot who has a supply. The latter, when he returns to the ground after a prolonged flight, will be fresh and able to start out anew, while the man who did not use oxygen will be tired out and unable to do any more that day and possibly the next. It has been proved that the squadrons at the front which used oxygen were six times as efficient as those which did not, and the use of oxygen is strongly recommended by the flight surgeons. ("Air Service Journal," October 17, 1918.)

"Giant" Aeroplanes.

The article describes types of "Giant" aeroplanes, and points out that Italy has preferred different types to those accepted by Great Britain, France, and Germany. The author first deals with the Caproni biplane, with three motors, having a span of 22 m. surface of 90 m^2, 320 h.p. motors, and is capable of lifting 1,000 kg.; this machine has two separate bodies. A larger Caproni machine has a surface of 125 m^2, 600 h.p. motors, and will lift 1,500 k.g. The Voisin triplane, the second type of which was built in 1916, has two bodies in the vertical 'plane, a total span of 36 m., surface of 200 m^2, 800 h.p. motors, and a lifting capacity of 2,000 kg. The Caproni triplane has a total span of 30 m., a surface of 180 m^2, 600 h.p. motors, and a lifting capacity of 2,000 kg. The Gotha-Lizenz, a German biplane type, has a span of 41 m. surface 314 m^2, four motors with a total power of 1,200 h.p., and a lifting capacity of 13,000 kg. Finally, the author describes the new American Langley machine with 300 m^2, surface biplane type, with 1,600 h.p. motors. All the machines are illustrated by photographs and line drawings. ("La Nature," November 30, 1918.)

Centrifugal Tachometers on the Principle of the Conical Pendulum.

The article is in two parts and describes five main types of centrifugal tachometers. The general theory of each type is given, and the relation between n, the revs. per minute, and α, the angular displacement of the arms of the conical pendulum, is worked out. There is for each type a variable β, such that $n=f(\alpha, \beta)$, and a series of characteristic curves of n against α are drawn for a range of the values of β.

(A). Conical pendulum with flat spiral spring control. In this instrument the shaft, whose revolutions are to be measured, carries two arms in the form of a cross, each of which can turn about its centre, *i.e.*, about an axis at right angles to that of the shaft. Each arm also carries a ball at each end. The couple due to the centrifugal forces on the balls is balanced by that due to the pull in a flat, spiral spring, whose plane is parallel to the two arms. The variable parameter β in this case is the angle α_0, which is the value of α when n is zero. The characteristic curves show that positive values of α_0 up to about 30° give the best range of measurable speeds, and the most open scale. α_0 is positive when the zero position (spring unstretched) is such that the spring is extended when the arms begin to move outwards from the axis.

The angular movement of the arms is communicated by means of a pair of links to a sleeve which slides on the revolving shaft. The axial displacements of the sleeve are transmitted by means of a connecting rod and crank to the scale pointer. An excellent uniform scale over a fairly wide range of speeds can be secured in this manner; the best results are obtained if the joint of connecting rod and crank lies above the crank centre when the shaft is at rest, assuming the axis of the shaft to be vertical.

(B). Conical pendulum with gravity load. This is an instrument on the principle of the Watt Governor. It can only be used for limited ranges of speed. Characteristic curves are shown in which the variable parameter is the ratio between the distance of the hinges of the arms carrying the balls from the axis of rotation and the length of the arms.

(C). Conical pendulum with linear spiral spring control. This scheme is the basis of successful types of accurate tachograph. The pendulum masses are joined directly by the spring, and the arms are hinged about the ends of a bar which is at right angles to, and turns with, the shaft whose revolutions are to be measured. The angular movements of the pendulum arms are communicated by a link mechanism to a sleeve on the shaft, and the sleeves are directly transmitted to the pen, which traces a record on the revolving drum.

The characteristic curves are drawn with the unstretched length of the spring as the variable parameter. They show that the principle is only applicable to cases where the speeds to be recorded are merely comparatively small variations from a constant mean.

(D). Centrefugal tachometers with differential gear drive. In these instruments the principle of the foregoing centrefugal tachometers, viz., a conical or rotating pendulum controlled by means of springs, is utilised, but the method of registering the deflection of the arms is essentially different. In outline the method is as follows: The displacement of the rotating pendulum masses produces a relative motion between two concentric shafts, one of which is connected to one side of the differential gear, and the other (a hollow shaft) is driven directly through bevel gearing from the main shaft. This hollow shaft has a speed of rotation equel and opposite to that of a second hollow shaft, also driven from the main shaft by bevel gearing, and connected to the other side of the differential gear. The above mentioned relative displacement of the concentric shafts can therefore be seen as a displacement of the axis of the two middle wheels of the differential gear with respect to the axis of the shaft,and this latter displacement is registered

by the indicator. The arms carrying the rotating pendulum masses have their hinges at opposite ends of the diameter of a rotating disc, mounted on the hollow shaft, and the axis of the hinges are also those of a pair of toothed wheels which gear with a pinion on the inner shaft. Any displacement of the arms gives rise to a turning of the toothed wheels, and therefore to a relative movement between the two concentric shafts.

If the centrefugal forces on the rotating masses are balanced by flat spiral springs, then the characteristic curves are similar in character to those of type A. There are no improvements in the characteristics as compared with type B when the two masses of the present kind of tachometer are joined by a linear spiral spring. An instrument can, however, be devised which gives an excellent linear relation between n and α and over a very wide range of speeds. The essential point of such an instrument is to choose suitably the position of the fixed end of a straight spiral spring, which is attached at its other end to one ball only. The axis of the spring should be roughly at right angles to the mean position of the arm carrying the ball.

(E). Centrifugal tachometers with combined spring and gravity loading. The example given of this type is a combination of types B and C, and its characteristic curves are certainly an approximate proportionality between n and α. both as regards range and approximate proportionality between n and α. ("Zietschift des Vereines deutscher Ingenieure," November 16 and 23, 1918.)

The Krell Manometer.

A modification of the ordinary U tube type of manometer is the Krell, in which one branch of the U is a glass tub, set at a small angle, and the other branch is a large tank. For various reasons the Krell manometer requires calibration. These are chiefly connected with the fact that glass tubing is generally not absolutely uniform in surface or bore along its length and may also not be exactly straight.

A. A. Merrill describes a manometer designed by himself, in which the reading is an observation of the movement of the sloping tube along its length required to bring it into the same position relative to the meniscus as for the zero, thus cutting out the errors due to the causes mentioned above. In order to avoid any change in level due to the movement of the tube connecting the sloping glass tube with the reservoir the former is made about two feet long. ("Aviation," November 1, 1918.)

Safeguarding the Health of Workpeople in Aeroplane Dope Shops.

The writer describes the composition of dopes and their solvents and outlines means to be adopted for securing the safety of operators using dope. The great danger arises from the vapours given off by the dope when applying it to the wings, etc., of aeroplanes. Three coats are generally applied with a brush, each coat being allowed to dry before applying the successive one.

The methods employed for trapping the vapours are very varied, but may be divided into three broad classes. In one, the wings or planes to be doped are arranged on the framework of large funnels, the opening of which is fitted with piping connected up with a small forge ventilator. The writer condemns this type. He remarks that, although the vapours are heavier than air, a single suction pipe is insufficient. The efficiency of the ventilator is also low. There is also great danger in allowing these vapours to circulate in the ventilator, owing to risk of fire. Further, it is uneconomical to allow products to escape into the air which are of commercial value.

In another type the ventilator sucks the vapours into the base of a metal

column filled with coke which is constantly being played upon by an atomiser. This is more economical, but has the same defects as the first.

In the third type suitable draught hoods or one or two high-efficiency helical fans are installed. This arrangement does not tend to economy, but is very efficient in that the entire air of the dope-room is breathable.

In conclusion, the writer suggests as the true solution the complete trapping of the vapours at the points where they are formed, and their recuperation, where possible. They should be trapped under a funnel or hopper, having at least three suction heads, with high-efficiency and low-depression ventilators placed at the end of the recuperator tower, and not between the hopper and the tower. Instead of having a single column of coke, the towers should be led to slabs on which are placed earthenware rings, and at least two towers should be working at a time. ("Annales d'Hygiène Publique," August, 1918; "Bull. de la Soc. d'Encouragement pour l'Industrie Nationale," September-October, 1918.)

Development of Air Traffic.

The article is the author's reply to the discussion which followed his Paper:

1. The regidity of the long girders which support the gas cells is ensured by making them not less than 25 to 30 feet deep.

2. With regard to the two supporting wheels at either end of the girders, it is pointed out that these wheels are provided with springs and rest in frames similar to the wheels of aeroplanes. If, therefore, the weight of the airship increases, the springs will yield and give additional points of support to the girders. There is, however, no reason why a larger number of wheels and springs should not be fitted, should this be found desirable.

3. Girders 25 to 30 feet deep should be of ample strength to carry uniformly distributed motors of 200 to 300 h.p., as motors of this size have not given any trouble or produced undue local stresses in the framework of slender and lightly-built aeroplanes.

4. There should be no difficulty in elevating the airship by means of propellers, as these do not in any way support the weight of the ship. The bouyancy of the airship will, in fact, cause it to rise without any help from the propellers. (A. P. Kapteuyn, "De Ingenicur," November 2, 1918.)

ABRIDGMENTS OF RECENT PATENT SPECIFICATIONS (AERONAUTICS).

COMPILED BY MR. H. T. P. GEE, PATENT AGENT.

120,603. H. Lord, 1, Chelmsford Street, Coppice, Oldham. November 8, 1917.

A gland or fairlead for an aerostat or other gas or liquid container to permit the passage through the envelope of a pressure-tube or a control cord, comprises two co-operating members which clamp the material of the envelope between them and carry one or more flexible discs through which the cord or tube passes. The drawings show two annular members with stepped surfaces clamped over the edges of a balloon envelope by a nut. Two rubber discs adapted to form a fluid-tight joint with the control-cord of a valve or ripping-panel or with the flanged tube are secured against one of said members by a nut. Cords secured to corks wedged through holes in a flange on the nut normally hold the cord in position, but become disengaged when the cord is pulled. The flexible discs allow the cord or tube to pass at any angle within the limits shown by dotted lines.

120,612. S. Boxell, 21, Hollingbury Road, Brighton. November 12, 1917.

Propelling.—An aircraft propeller has blades arranged to revolve round a central axis and these blades are geared in a particular manner so as to obtain lifting or propelling forces only or combined lifting and propelling forces. The driven shaft mounted in ball bearings carries frames in which blades are pivoted. Each blade carries a worm-wheel which is driven by a worm mounted on a shaft capable of sliding and caused to rotate by the pinion which is always in gear with a toothed plate fixed to the framework. Longitudinal motion is imparted to the shafts by normally stationary cams adapted to be moved along another shaft by a lever. One of the cams is for obtaining a combined lifting and propelling force, while the other cam is used for lifting only or propelling only according to its position.

120,627. P. F. Chaplin, The Grange, Beaconsfield, Buckinghamshire. November 15, 1917.

Screw Propellers.—In laminated propellers for aircraft, the joints of the laminæ forming a blade are at different radial distances from the axis of the propeller, and there is no straight line of joints across the blade.

120,694. H. T. Brereton, 3, St. Albans Road, Hatfield, Hertfordshire. March 21, 1918.

Clinometers.—An instrument for use on aeroplanes comprises a vertical counterbalanced pointer pivotally mounted in front of a graduated dial fixed in a casing provided with lugs for attachment to an aeroplane. In the form shown, the weight for counterbalancing the pointer is attached to the pointer itself, but in a modification it is attached to a rod fixed to the spindle. The ends of the pointer are of contrasting colours and in the normal position are over spaces of contrasting colour on the dial.

120,701. British Westinghouse Electric and Manufacturing Co., 2, Norfolk Street, Strand, Westminster. (Westinghouse Electric and Manufacturing Co., East Pittsburgh, Pennsylvania, U.S.A.) May 23, 1918.

Cars; Framework; Planes, Construction and Arrangement of.—The various parts of aeroplanes are made of fibrous material, such as wood, asbestos paper,

cardboard, or cloth, impregnated with condensation products of phenols and formaldehydes as described in Specifications 1921/08 and 21566/08, or of other similar condensation products, wrapped, pressed, or moulded into the required shape, and hardened by heat-treatment. The drawings show an aeroplane having the body formed in one piece in the manner described above, the rear end being strengthened by cross-bracing and having fins formed integrally therewith. The fins are provided with braces and are covered at the rear ends by a cruciform cap to which the rudder and elevators are hinged. The rudder and elevators are composed of a front support of suitable material, to which and to a trailing edge are secured the surfaces formed of the above-mentioned materia's and with inwardly-projecting ribs. The main planes are formed as described above of tubular or I-section spars threaded through ribs separated by tubular spacers and connected to a leading edge and trailing edge. The whole is covered with fabric. The interplane struts are formed by winding layers upon a mandrel and are connected to the planes by screwing on to members attached to metal brackets seated on the spars. Other forms of stream-line struts are described. The cables may be enclosed in stream-line casings of similar form.

Land Wheels and Skids.—Wheels carried by braces and a skid are formed of the above-mentioned materials. The skids are fitted with shock-absorbers.

Steering.—The operating-arms of the rudder and elevators are formed of hollow sections attached by bolts.

Screw Propellers.—The propellers are formed of layers of fabric-paper or like material impregnated as described above, placed one upon another in a mould of the required form, and baked; or the material may be filled into the mould in pulp or shredded form.

120,742, E. A. Edwards, Ivy Lodge, Southwick, Sussex. November 21, 1917.

Aerial Machines without Aerostats; Planes, Arrangement of; Propelling; Steering.—An aerial machine is supported by one or more propellers arranged along the centre line of the machine, each propeller comprising two planes normally set at opposite angles but capable of being adjusted individually by winches to act as supporting planes when the machine is gliding or when driven horizontally by one or more screw propellers. The planes are mounted on horizontal spindles on sleeves surrounding vertical shafts which are driven through bevel-wheels from the transverse shafts geared by ropes to the transverse shaft of a petrol or other engine. The vertical shafts can be coupled to and uncoupled from certain of the bevel-wheels by sliding clutches operated by levers.

Floats, Arrangement of.—The machine is supported on two longitudinal tubular floats of circular, elliptic, or pear-shaped section.

120,743. H. J. Guise, 24, Craven Park Road, Harlesden, London. February 28, 1918.

Aerial Machines with or without Aerostats; Propelling; Steering.—A propelling unit for aircraft comprises a screw propeller rotating within an annular shell carried by spider arms and links from a tube which is connected to casings providing bearings for the shaft driven by a motor. One of the casings is fixed to the motor and mounted on trunnions about which the unit as a whole may be adjusted. The drawings show a machine having a central unit fixed with its shaft vertical, four units adjustable about trunnions and an horizontal tube fitted with a tractor unit. The units are driven by electric motors and are adjusted by means of levers connected by rods to cranks on a shaft. An auxiliary engine is provided for starting the main engines and for driving an electric generator. Supporting planes or a gas envelope may be provided. The propelling units may be used for raising aeroplanes rapidly.

120,765. R. D. England, 100, Richmond Road, Montpelier, Bristol. November 20, 1917.

Aircraft Propellers, etc.—Relates to the manufacture of the propellers and other parts of aircraft. A full-sized model of the propellers, etc., is made in clay, plasticine, etc., from which a split or two part reinforced concrete or cement mould described hereafter is prepared. This mould is greased and receives a shell of papier mâché and cotton fabric or canvas which is filled with a composition of cotton wool, vegetable fibre, or other material such as sawdust or hair mixed with a spirituous solution of shellac, the whole being reinforced by metal. After the propeller, etc., is moulded and dried, it is plunged into a hardening ammoniacal solution or is subjected to the influence of a vapour of ammonia and steam and the surface may be glazed while moist by the action of fluo-silicic acid gas and is afterwards polished, stoved, or coated with metal. Apparatus for carrying out the invention is described. The drawings show an iron wire skeleton for holding the plastic material for the blades, and a mould in two parts hinged and secured by a fly-nut.

120,777. E. Cragg, 124, Joy Street, Syke, Rochdale, Lancashire. November 22, 1917.

Steering and Balancing; Propelling; Planes, Construction of.—An aeroplane has the centre portions of its planes adjustable in incidence and its tips variable in area, and has two propellers adapted to be coupled in various ways to either or both of two engines.

120,788. F. W. Chamier, 59, Jermyn Street, London. November 29, 1917.

Aerial Warfare.—Consists in protecting an area against aircraft attack by means of high-tensile steel wires or cables spaced apart and supported by balloons or kites. The wires are paid out from winches and may be provided with parachutes and hooks to help the wires in upsetting or destroying enemy aircraft coming in contact with the wires. The balloons and parachutes may be painted white or light grey underneath and black or dark grey above to make them less visible in daylight; for the same purpose at night, the colouration is reversed.

120,791. R. G. Foot, 11, Queen Victoria Street, London. December 1, 1917.

Propellers.—An air propeller for aeroplanes, ventilating fans, etc., has on the driving face of each blade a number of concentric ribs.

120,824. W. J. Mellersh-Jackson, 28, Southampton Buildings, London. (India Rubber Co., 1790, Broadway, New York, U.S.A.) January 2, 1918.

Coloured Rubber.—A sheet of rubber or rubber fabric is provided with pigment (*e.g.*, aluminium powder) in its outer layer only, the pigment being embedded by vulcanisation so as to produce a smooth glossy surface. Thus, a sheet of vulcanisable rubber mixing may be sprinkled with a dry powdered pigment without incorporation, and metal foil or other confining surface placed over the pigment preparatory to vulcanisation. The rubber and foil may be superposed in a succession of layers, *e.g.*, by reeling into a compact roll, to facilitate handling. The product may be employed as a balloon or like fabric or as a wing, boat, or pontoon material for aeroplanes.

120,849. H. A. Grandjean, 32, Rue de la Folie Regnault, Paris. April 17, 1918.

Screw Propellers.—In order to produce a vacuum in front of the screw propellers of aircraft, ships, and air and marine torpedoes, each propeller is given a reciprocating movement in addition to the rotary movement by means of a cam or ramped dics supported by the block and bearing against a cross-head fixed to the propeller shaft. Ball bearings are fitted between the cam and the cross-head.

120,885. H. Bland, Ashley Street, Waverley, near Sydney, Australia. October 23, 1918. Convention date, October 11, 1917. Not yet accepted. Abridged as open to inspection under Sect. 91 of the Act.

Screw Propellers.—The pitch of propeller blades is varied or reversed by means of a solid shaft capable of turning in a driving sleeve which is flanged to form one member of a two-part hub. The shank of each blade has a worm-wheel which gears with the common worm at one end of the solid shaft, the other end having a spur wheel in gear with a pinion, the shaft of which carries a spur wheel engaging a pinion on a brake sleeve, loose on the sleeve. The engine shaft carries a fixed disc which drives by means of pins the disc on the sleeve. Braking the loose brake sleeve causes the train of gearing described to turn the shaft independently of the sleeve so as to alter the pitch of the blades.

120,916. Naamlooze Vennootschap Nederlandsche Automobiel en Vliegtuigfabriek Trompenburg, Amsterdam, Holland. November 18, 1918. Convention date, November 18, 1917. Not yet accepted. Abridged as open to inspection under Sect. 91 of the Act.

Wire Couplings.—Flat steel bracing-bands for aircraft and other structures are secured by bending them over wedge-shaped blocks which are slid sideways into tapered recesses and held in place by sleeves. The drawings show two bands bent over a wedge and into a slot in the base of the wedge. The bands and wedge are inserted between the tapered jaws of a fork and a tubular sleeve is slid over a turned part of the fork. The wedge may be in two parts to distribute the load on the two bands.

120,929. J. W. Rapp, Flushing, New York, U.S.A. (Assignee of J. Vanorio, Brooklyn, New York, U.S.A.) November 22, 1918. Convention date, November 19, 1917. Not yet accepted. Abridged as open to inspection under Sect. 91 of the Act.

Planes, Construction of; Framework.—A plane rib or other aircraft part is formed from two channel-section steel members braced by struts and diagonal members also of channel-section, all welded together at the joints. All the members have parts cut away. The diagonal braces are made in pairs, the flanges being cut away to form abutting surfaces at the bend. The ends of the struts are cut to fit within the bends and are compressed laterally to fit within the flanges of the diagonal members. The ends of the steel members are welded to tubes.

120,939. Blackburn Aeroplane and Motor Co., Olympia, Roundhay Road, and S. Wilson, 47, Markham Avenue, Harehills, both in Leeds. August 18, 1917.

Aircraft Propellers.—In assembling the glued laminæ of aircraft propellers, they are registered in the required form and are clamped together while the glue is setting between upper and lower step-like structures supported respectively by a pressure arm or head and by a bench. The arm is pivoted to an adjustable post supported by one end of the base, and a screw-threaded rod is pivoted to the opposite end of the base and is provided with a nut by which pressure is applied to the arm. A number of these clamps are applied at intervals along the laminæ.

120,942. S. G. Rowles, Kethro, St. John's Road, Orpington, Kent. March 6, 1918.

Clinometers.—A gyroscopic clinometer for aircraft, submarines, etc., comprises a graduated sphere suspended in gimbal rings and weighted with a gyroscope. A pointer is fixed to a bridge on the ring, which is adjusted by screws.

120,943. H. Wilson-Fox, 20, Lowndes Square, London. October 24, 1917.

Aerial Machines with Aerostats; Aerostats.—An aerial machine comprises an aerostat, supporting planes associated with the aerostat, and means for utilising the gas of the aerostat as fuel for the engine in addition to the usual fuel. The aerostat is rigid or semi-rigid, with ballonets connected to a common pipe provided with a valve for controlling the flow of gas to the engine. An air pump may force air into the envelope as the gas is used. The planes may be of the monoplane, biplane, or triplane type, and are capable of supporting the machine when all the gas is used. The car may be of the boat-like form and detachable.

121,006. C. W. Taverner and D. J. Mooney, 48a, Gillingham Street, London. December 21, 1917.

A metal spar for wings, ailerons, etc., has thin metal flanges united by webs in the form of vertical rectangular tubes. The drawings show channel-section flanges adapted to be riveted to web-sections formed by bending thin metal to form three closed sides, from two of which tongues are cut and are either bent over and secured together or riveted to the next section. Projections, left when the tongues are cut, overlap and are riveted to the adjacent section. Flanged holes and embossed parts are formed in the sides of the webs for lightness and strength. In a modification web-sections have flanges riveted to flanges of the form shown.

121,020. J. Moston, Mount Pleasant, Booth's Hill, Lymm, Cheshire. January 18, 1918.

Aerial Machines with Aerostats; Aerostats, Balancing; Cars.—An aerial machine, comprising a gas-container, to the side frames of which are connected pivoted supporting planes and swivelling propellers, has cars mounted at each side capable of longitudinal swinging movements which control the inclination of the planes and propeller shafts. Manual adjustment of the planes and propellers is also provided for. The drawings show a car suspended on cranked portions of transverse shafts, the cranked parts being coupled together by rods. A rod on the upper part of the car actuates cranks on the lowest of a series of shafts, the lowest shafts being coupled by the cranks and coupling rods shown to shafts vertically above them.

121,032. S. E. Saunders, Ltd., Columbine Works, Medina Road, and W. G. Woodford, Rushmore Villa, Victoria Road, both in Cowes, Isle of Wight. February 5, 1918.

Wire-Strainers.—A locking device for aeroplane and like stay wire-strainers, comprising a screwed sleeve engaging right and left-handed screwed rods, consists of a U-shaped sheet metal cap pivoted to the sleeve and adapted to embrace the eye of one of the screwed rods. The locking member is cut away near the pivot to allow it to be turned so that it may be employed in rotating the sleeve. In a modification, the locking member may be bent to stream-line form and may be duplicated to engage both screwed rods.

121,045. R. H. Annison, 16, Water Lane, Great Tower Street, London. February 28, 1918.

Darts for Airmen.—Consists in making a dart for airmen to drop from aircraft with a cylindrical or oval body, a direct-impact nose fuse, a central fuse tube, and a small explosive charge for blowing off the top and igniting an incendiary, illuminating, spark-producing, or smoke-producing charge. Streamers are provided to ensure the vertical fall of the dart. The striker of the fuse is held in place by grooves and studs, resilient gripping tongues, bayonet joints, or transverse safety pins. A float is added when the dart is to be dropped in water.

121,056. N. L. Hyatt and H. M. Lloyd, Dresden, Ontario, Canada. March 19, 1918.

Aerial Machines without Aerostats; Propelling.—An aeroplane provided with a number of lifting propellers arranged above the planes on opposite sides of the machine and rotating in different directions has the propellers driven from a vertical shaft adapted to be clutched to the main motor. This shaft extends through a tubular standard secured to the top of the fuselage and drives through the bevel-gear shown four diagonally radiating shafts arranged in tubes secured to gear casings supported by stays. The propeller shafts are driven by bevel-gear, the wheels on the other pair of shafts engaging the wheels on the opposite sides of the shafts so as to drive that pair of propellers in the opposite direction. The standard and stays applied to a biplane extend through the upper plane.

121,072. W. A. Wilson, 62, Fortunegate Road, Harlesden, London. April 25, 1918.

Propelling.—In aeroplanes, hydro-aeroplanes, airships, etc., provided with paddle-fans at each side driven in synchronism with the main tractor or propeller, the paddle-fans are rotated by cranks and connecting rods engaging a cam race secured to the tractor, and a screw is provided in front of the tractor and adapted to force air rearwards against cups or the like carried by the fans. Air driven by the fans is directed under the main planes or against deflectors for increasing the lift. The inner surface of the paddle-boxes may be lined with cork, rubber, etc., with a view to deadening the sound.

Planes, Construction of.—The under surface of the main planes also may be covered with cork, rubber, etc., for the purpose stated above.

Parachutes.—A parachute device comprises a funnel fitted with flaps and leading to a pipe, which is closed at the top or opens under the main planes.

Aerostats.—In the case of an airship, air may be passed from pipes into a bag or envelope during a descent.

121,300. F. Sage and Co., and E. C. G. England, 58, Gray's Inn Road, London. July 5, 1917.

Raising Liquids.—Apparatus for raising liquid comprises a pump which produces pulsation in a column of air in a pipe and causes liquid to rise up another pipe and pass through valves into a chamber. On reaching a certain level, the liquid, through a float, lifts one of the valves and tends to stop the supply of liquid; but if this level is exceeded, the float closes an air valve and produces an air cushion in the chamber. The chamber may be primed through a funnel. The apparatus is designed to supply liquid fuel, water, or lubricating oil to the engines of aircraft, etc. The pump may be driven by a wind motor or by the engine; it may be of any type.

121,378. F. W. Chamier, 50, Jermyn Street, and W. F. Howard, 104, The Avenue, Ealing, both in London. January 2, 1918.

Planes, Arrangement and Construction of; Propelling.—The aerofoils of aeroplanes, etc., are so arranged that, when the machine is on the ground or climbing, the chords of the aerofoils have an angle of incidence which is positive but less than the angle between the line of propeller thrust and the ground, so that the aerofoils have a negative incidence when the machine is in flight with the propeller thrust horizontal. The aerofoils are preferably cambered so that the sine of the angle between the propeller thrust and the chords of the aerofoils is equal to the ratio of the maximum ordinate of the camber to the chord of the aerofoil.

121,389. H. C. Dickson, Glengarth, Quinton, Birmingham. January 31, 1918.

Metal Tubes, etc.—Tubes, stays, struts, etc., are constructed of sheet metal bent into the required cross-section and formed with internal ribs adapted to hold one or more internal cross-webs, the invention being also applicable to the planes, body, and other parts of aircraft. As applied to the planes of aerial machines, the tube may be shaped to the usual camber, but in the case of tail planes, may be of symmetrical shape. According to the Provisional Specification, the upper and lower faces of such a tail plane may be extended forwards to constitute a sheet metal body for the accommodation of the pilot.

121,393. G. C. St. Louis and C. Pearson, Fresno, California, U.S.A. February 6, 1918.

Aerial Machines without Aerostats; Planes, Construction and Arrangement of; Cars; Propelling.—An aerial machine comprises a car fitted with wheels adapted to be driven from the engine and hung by rods from a frame upon which is mounted a box-plane with its greater dimension in the direction of flight, and a propeller. The sides of the plane are inclined inwards from the bottom upwards, the inclination gradually increasing from the front to the rear, and side flaps are fixed along the top edges. The sides extend below the bottom surface of the box-plane so as to form horizontal lower edges, and a central fin having an horizontal upper edge is fixed on top. The bottom surface is formed with openings, which may be more or less closed by flaps.

Steering and Balancing; Stopping Way; Parachutes.—The box-plane is adjusted in inclination about pivots and at the same time is moved longitudinally to maintain its centre of pressure vertically above the centre of gravity. The pivots are carried by rods movable longitudinally in bearings and fixed to hangers engaged by worms, which also gear with pinions of different diameters mounted on screwed shafts. These shafts engage sleeves provided with pins engaging slotted guides fixed to the lower edges of the plane. The worms are operated by means of bevel-gear, chain-gear and hand-wheel. The area of the bottom surface of the box-plane is varied by flaps normally pressed away from openings by springs, but adjustable over the openings by means of cables and levers. A rear elevator is operated by means of a cable and lever. The rudder is adjusted simultaneously with the front land wheels by means of a steering column. Side planes hinged within the box-plane may be swung inwards, either together for stopping way or for acting as a parachute in the case of a sudden descent, or singly, for use as rudders. The movement of the side planes is effected by means of rack-and-pinion gears coupled by chain gears actuated by means of shafts, bevel-gears and hand wheels.

Land Wheels; Shock of Landing, Deadening.—The land wheels are carried by springs. The rear wheels may be driven through a clutch and shaft, and the front wheels may be steered as described above.

121,411. G. H. Thomas, 27, Buckingham Gate, and E. J. Edgar, 16, Teignmouth Road, Brondesbury, both in London. March 8, 1918.

Wheel Tyres; Tyre Attachments to Rims.—Pneumatic tyres, particularly for use on wire wheels of aeroplanes, are formed with beaded edges comprising hard rubber cores in which are embedded flat zigzag wires.

121,459. E. Caretta, 30, Via Legnano, Turin, Italy. November 1, 1918 Convention date, December 12, 1917. Not yet accepted. Abridged as open to inspection under Sect. 91 of the Act.

Girders; Aircraft.—An aluminium or other light metal girder, chiefly for aircraft, is formed with longitudinal ribs on the inner face of both flanges and

either on or close to the edges. The ribs may be inclined inwards or outwards, or may be zigzag in section. The girders are made by extrusion through a die having the cross-section desired.

121,461. H. Schneebeli, 28, Rue de Becon, Courbevoie, Seine, France. November 12, 1918. Convention date, December 8, 1917. Not yet accepted. Abridged as open to inspection under Sect. 91 of the Act.

Internal Combustion Engines; Two-Stroke Cycle Engines.—In an engine having a pair of communicating cylinders with pistons acted upon simultaneously by each impulse, air is compressed in a common crank casing and is carburetted with fuel admitted through a spraying nozzle located in a transfer passage. Air enters the crank casing through a non-return valve, and the non-carburetted portion of the air compressed in the transfer passage serves as a scavenging charge. A separate pump may supply the scavenging charge.

121,462. B. Graemiger, Zurich, Switzerland. November 13, 1918. Convention date, December 10, 1917. Not yet accepted. Abridged as open to inspection under Sect. 91 of the Act.

Centrifugal Pumps; Multistage Centrifugal Fans and Pumps.—The shaft is reduced in diameter between the seats of adjacent impellers and the reduced surface forms part of the inlet channel to the second of the said impellers.

121,463. Soc. des Moteurs Salmson (Système Canton-Unné), 74, Rue St. Lazare, Paris. November 27, 1918. Convention date, December 11, 1917. Not yet accepted. Abridged as open to inspection under Sect. 91 of the Act.

Propelling.—A frame for supporting an engine in an aeroplane fuselage comprises two plates connected together by peripheral channel bars. Recesses are provided to receive the longerons, and the engine is secured by bolts passing through holes around the central opening.

Cars and Cabins.—Lugs may be provided for attachment of the fuselage stay wires.

121,464. Soc. des Moteurs Salmson (Système Canton-Unné), 74, Rue St. Lazare, Paris. November 27, 1918. Convention date, December 11, 1917. Not yet accepted. Abridged as open to inspection under Sect. 91 of the Act.

Steering and Balancing; Cars and Cabins; Framework.—The pivotal members for the elevators and rudder of an aeroplane are supported by a vertical tube welded to sockets which receive the rear ends of the fuselage longerons, the sockets being held in position by the fuselage stay wires. The drawings show the sockets welded to a vertical tube and held on the ends of the longerons by stay wires. The tubular shaft for the elevators is carried by bearings supported by a cross-tube passing through and welded to the tube. The rudder is pivoted on the tube by means of pivots. Inclined tubular braces and stay wires are arranged as shown.

121,465. Soc. des Moteurs Salmson (Système Canton-Unné), 74, Rue St. Lazare, Paris. November 27, 1918. Convention date, December 12, 1917. Not yet accepted. Abridged as open to inspection under Sect. 91 of the Act.

Framework.—A metal fitting for connecting together the wing spars and interplane struts of aeroplanes comprises a member through which the spar passes and to which it is secured by bolts, the strut being received in a socket. The pins of the stay wires are received by lugs formed by a metal plate, folded and welded as shown. Other wiring lugs are formed of metal plates bent over at their outer edges and provided with holes adjacent these edges. Further, the socket is extended for the attachment of other stay wires.

121,466. Soc. des Moteurs Salmson (Système Canton-Unné), 74, Rue St. Lazare, Paris. November 27, 1918. Convention date, December 12, 1917. Not yet accepted. Abridged as open to inspection under Sect. 91 of the Act.

Framework.—A metal junction piece for connecting fuselage longerons and cross-struts, cabin stanchions, wing spars, and under-carriage struts of aeroplanes comprises a three-sided box-shaped portion mounted astride and bolted to the longeron formed with flanged lower edges for the attachment of flanges on the under-carriage struts and with socket for the attachment of the cabin stanchions, and fitted with a socket member welded in position and adapted to receive the fuselage cross-strut, the wing spars being attached to lugs on the socket. The interplane bracing wires are attached to lugs formed by a folded plate welded in position, and the under-carriage bracing is attached to lugs provided on the flanges.

121,467. Soc. des Moteurs Salmson (Système Canton-Unné), 74, Rue St. Lazare, Paris. November 27, 1918. Convention date, December 13, 1917. Not yet accepted. Abridged as open to inspection under Sect. 91 of the Act.

Planes, Construction of; Framework.—A metal fitting for connecting the ribs of aeroplane elevators to the pivot tube comprises a socket which is secured by pins to the pivot tube and formed with laterally-extending wings for bolting to the ribs.

121,468. Soc. des Moteurs Salmson (Système Canton-Unné), 74, Rue St. Lazare, Paris. November 27, 1918. Convention date, December 13, 1917. Not yet accepted. Abridged as open to inspection under Sect. 91 of the Act.

Framework.—A metal fitting for connecting longitudinals to uprights and cross-members of aeroplane framework comprises a metal plate bent so as to provide a part connected by bolts to the longitudinal, a part attached by screws to the upright or cross-member, and inclined lugs for attachment of bracing wires. The lugs of two adjacent fittings may be connected to the same bracing wire. The upright and cross-member are formed at their ends with slots to fit over the heads of the bolts for preventing slipping. The outer edges of the lugs may be folded over on themselves.

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Military Observation Balloons. By Emil J. Widmer. London, 1918: Crosby, Lockwood and Son. Plates and Illustrations. pp. 151 oct.

This book is apparently based on a translation of a German manual on Kite Ballooning.

It gives much detail of the organisation and drill of a Balloon Company and a general description of the construction of the balloon and its gear.

The chapters on fabric and materials do not give sufficient details of any great scientific value to anyone who is already familiar with work of this nature. The following paragraph may be quoted:—

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> "*The Weight of the Rubberization.*—In order to determine the weight of the rubberization, one must first know the weight of the cloth when dyed; after taking the weight of the material (in double-ply material the weight of the outer and inner layer) from the weight of the finished product, the remaining weight is that of the rubber, which is then judged by the unit area."

The work will, it is thought, be mainly of interest to those who are already familiar with modern balloon practice; they will derive considerable pleasure in comparing their own experience with the picture of the German arrangements previous to the war.